THREE ESSAYS IN APPLIED ECONOMICS: AN EVALUATION OF BRAZILIAN PUBLIC POLICIES

BY

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DISSERTATION

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ABSTRACT

My dissertation is composed by three essays with ex-post evaluations of public policies adopted in a major country of the developing world: Brazil. In each essay, I have tried to identify the main outcomes of the evaluated policies using quantitative econometric methods. Ultimately, the objective of these studies is to contribute to the policy debate by providing a credible evidence of policy outcomes and limitations.

My first chapter investigates how fare-free public transportation for the elderly affects the travel behavior and well-being of policy beneficiaries in major Brazilian metro areas. The second essay of my thesis analyzes how a series of changes in urban speed limits affected road accidents and travel time in São Paulo, Brazilian largest city. Last, my third chapter studies the expansion of affirmative action policies in Brazilian Federal universities, measuring how the adoption of these policies have changed the enrolment of students from targeted underprivileged groups.

In each of these essays, I provide novel evidence about the impacts of the evaluated policies. On my first chapter I present evidence that the speed limit reductions adopted in São Paulo have had a substantial impact on road accident reduction, and the welfare benefits associated with this outcome largely outweighs the welfare costs imposed by the policy restrictions due to longer travel times. The second chapter shows that the adoption of affirmative action by Brazilian universities increased the enrollment of underprivileged students, however, while race-conscious policies led to the enrollment of more Black students, race-blind affirmative action had no significant impacts on the enrollment of racial minorities. The third chapter shows that the policy of fare-free public transportation has a very limited effect on the use of private vehicles by beneficiaries, so a price incentive to public transportation may not be an effective tool for reducing car-related externalities.

Next, I present the individual abstracts for each of my thesis chapters:

Chapter 1: Assessing the Impacts of Speed Limit Changes in a Metropolitan Area of the Developing World

This paper assesses the outcomes of a set of policies that changed traffic speed limits in a major city of the developing world. We analyze policy impacts using data about traffic accidents, real time congestion scrapped from a web API, and traffic monitoring cameras. By exploring the temporal and spatial heterogeneity of policy adoption, we estimate its impacts on road accidents, traffic volume and commuting time. Our results indicate that speed limit reductions led to a large decrease of accidents on treated road segments while not affecting traffic volume. However, we also found that speed limit increases on major urban highways reduced the travel time on those roads. We then compared the social costs and benefits of a speed limit reduction. We calculate that the welfare gains associated with the reduction in road accidents are at least 46% larger than the welfare losses due to longer commuting times. Moreover, while the losses in commuting time are increasing with wealth, the benefits of accident reductions are proportionally larger for middle- and lower income individuals, thus indicating that a speed limit reduction have a progressive welfare impact in the setting evaluated in our study.

Chapter 2: Affirmative Action in Brazilian Universities: Effects on the Enrollment of Targeted Groups

This paper investigates how the adoption of affirmative action for college admission affected the enrollment of students from socioeconomically disadvantaged groups in Brazil. We explore the time heterogeneity of policy adoption by Brazilian universities to identify the policy effects while accounting for time confounding unobservables. Our analysis indicate that the adoption of affirmative action policies led to an increase in the enrollment of students from groups explicitly targeted by each policy, particularly public high-school students and Blacks. As for the heterogeneities of policy impacts, larger effects were observed for more competitive and more prestigious academic programs. Lastly, we found that universities that adopted affirmative action policies with explicit racial criteria experienced an increase in the enrollment of Black students; meanwhile, universities that adopted race-blind policies had no significant changes in the racial profile of their admitted students.

Chapter 3: Travel Behavior Effects of Fare-Free Public Transportation for the Elderly in Brazilian Metropolitan Areas

This paper investigates how a policy that grants fare-free public transportation for the elderly affects the travel behavior of its beneficiaries. To identify the policy impacts, I explore the fact that eligibility for exemption is based on age thresholds that vary by gender and by city. Using data from household travel surveys, I estimate the causal effects of free-fare on travel behavior through a Regression Discontinuity Design. The results of my analysis indicate that eligibility for fare-free public transportation increases transit ridership among beneficiaries in approximately 27.3%. However, I do not find any significant effects of the policy on mode substitution nor on trip characteristics.

To Professor Werner Baer

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Chapter 1: Assessing the Impacts of Speed Limit Changes in a Metropolitan Area of the Developing World

With Amanda Ang and Peter Christensen

This paper assesses the outcomes of a set of policies that changed traffic speed limits in a major city of the developing world. We analyze policy impacts using data about traffic accidents, real time congestion scrapped from a web API, and traffic monitoring cameras. By exploring the temporal and spatial heterogeneity of policy adoption, we estimate its impacts on road accidents, traffic volume and commuting time. Our results indicate that speed limit reductions led to a large decrease of accidents on treated road segments while not affecting traffic volume. However, we also found that speed limit increases on major urban highways reduced the travel time on those roads. We then compared the social costs and benefits of a speed limit reduction. We calculate that the welfare gains associated with the reduction in road accidents are at least 46% larger than the welfare losses due to longer commuting times. Moreover, while the losses in commuting time are increasing with wealth, the benefits of accident reductions are proportionally larger for middle- and lower income individuals, thus indicating that a speed limit reduction have a progressive welfare impact in the setting evaluated in our study.

Keywords: Speed Limit Changes, Road Accidents, Travel Time

Traffic accidents represent an astonishing cost to societies. Road injuries are the leading cause of unnatural deaths worldwide, with approximately 1.3 million fatalities per year, or 27.1% of all human unnatural deaths (WHO, 2015). Lack of road safety is particularly problematic in the developing world. Although low- and middle-income countries account for only half of world vehicles, 90% of road fatalities occur in those countries. Meanwhile, it is well established in the transportation literature that vehicle speed is strongly associated with the probability and severity of road accidents (Archer, Fotheringham, Symmons, & Corben, 2008), (Elvik, 2013). Therefore, speed limit regulations are adopted throughout the world, and in recent years, several major cities have been experimenting with stricter speed restrictions in urban roads.[1] However, lower speed limits may pose an important welfare cost to societies as commuters may face longer journeys. Ultimately, due to the complexity of urban transportation systems, weighting the costs and benefits of stricter speed regulations within cities is an empirical question which has not been fully addressed yet.

Within this context, this paper investigates the outcomes of a series of policies that altered speed limits in a major city of the developing world, namely São Paulo, Brazil. In 2015, the city government enacted a policy program reducing the speed limits in the city highways (*Marginais*) and arterial roads. However, in 2017 the speed limit of urban highways was reverted to pre-reduction levels. We explore the time heterogeneity of these changes to identify the policy impacts using combined datasets of traffic accidents, traffic cameras, and real-time congestion scrapped from a web API.

Our results indicate that the 2015 speed limit reductions decreased road accidents in about a third on treated road segments. Meanwhile, after the 2017 speed limit increase, travel speed on treated highways increased by about 6%, although effects were concentrated in off-peak hours. Given these results, we compare speed limit changes welfare costs and benefits. Using standard parameters from the literature for the value of time and the value of statistical life, we find that, in the case of speed limit reductions, the welfare gains associated with fewer accidents outweigh the welfare losses due to longer commuting by approximately 1.46 times. Finally, we explored the distribution of policy costs and benefits along distinct segments of society. We identified that while welfare losses due to travel time were larger for wealthier individuals, the benefits of accident reductions were concentrated on lower- and middle-income people.

Other empirical studies have evaluated the impacts of speed limit changes, for example (van Benthem, 2015) and (Ashenfelter & Greenstone, 2004), which analyzed the impacts of speed limit increases on American highways in the 1980s and 1990s. Our paper extends beyond these

[1] Examples include the cities of New York, Boston, London, Paris and Stockholm (Archer, Fotheringham, Symmons, & Corben, 2008),

studies as we analyze the effects of speed limit changes within an urban setting, where more than two thirds of total vehicle miles take place (FHA, 2016). Moreover, we analyze a metropolitan area of the developing world where road accidents and fatalities are a more prevalent problem. Finally, due to the richness of our data, we were able to explore the distributive impacts of speed limit changes, an aspect of this problem not yet explored in the literature.

With respect to our analysis of policy impacts on travel time, our study has the novelty of combining a citywide sample of representative trips with a web routing service that collects real time congestion information. By simulating the set of representative trips before and after a speed limit change, we were to identify in a finer scale the distribution of policy impacts on individuals. While other studies have already used real-time web routing services to evaluate the impact of urban transportation policies (Hanna, Kreindler, & Olken, 2017), to the extent of our knowledge, none have yet combined this approach with a representative sample of commuters to investigate the distribution of policy impacts throughout the society.

We are aware of at least one other study (Jardim, 2017) which had investigated the impacts of the São Paulo speed limit reductions on speed and road accidents. Our paper differs from that study as we investigate a larger set of roads,[2] use real time and daily measures of traffic speed and combine the reduced form results with a representative sample of the population to investigate the distributive impacts of the policy.

The remaining of this paper is divided as follows: Session 1 describes the background of the speed limit changes investigated in our study. Session 2 details the datasets used for our analysis, and Session 3 presents the empirical strategies used to identify the policy impacts on accidents and travel time. On Session 4, we compare the costs and benefits of the policies, including our distributive analysis. Session 5 compares our results with the literature and Session 6 concludes.

[2] Nevertheless, the reduced form results of speed limit effects on accidents from Jardim (2017) are of similar magnitude as ours.

1.1 Background

With more than 20 million residents, São Paulo is one of the largest metropolitan areas in the world. The city is characterized by high urban density, limited transportation infrastructure, and high spatial concentration of the economic activity (LSE - Urban Age Programme, 2009). Every day, approximately 30 million motorized trips are made in São Paulo, out of which about half are made by public transportation and half by private modes (METRO, 2013). All these factors combined cause the city of São Paulo to be one of the most heavily congested urban areas in the world. Table 1.1 presents some figures about the overall characteristics of motorized trips made in São Paulo on a regular weekday.

Table 1.1: Descriptive Characteristics of Motorized Trips Made in a Regular Weekday in São Paulo

	Trips per Day (Million)	Share	Mean Distance (km)	Mean Duration (minutes)
Motorized trips	29.74	1.00	7.99	50.53
By mode				
Bus	11.78	0.40	6.83	58.73
Rail	4.36	0.15	16.61	88.56
Car	12.49	0.42	6.02	31.46
Motorcycle	1.04	0.03	8.50	27.86
By motivation				
Work	16.81	0.57	9.88	60.25
Education	7.51	0.25	4.73	35.72
Other	5.43	0.18	6.90	43.14

Notes: data from the 2012 Mobility Household Survey of São Paulo (Pesquisa de Mobilidade Urbana 2012)

1.1.1 Speed Limit Reductions in 2015

From 2005 to 2014, there were on average 2,500 road deaths per year in the Metropolitan Area of São Paulo. This amount corresponds to a ratio of 12.5 deaths per 100,000 residents (DATASUS, 2018), and is 56.2% higher than OECD average. Given these figures, the government of São Paulo joined the World Health Organization Decade of Action for Road Safety" establishing the goal of reducing road mortality in the city to 6 deaths per 100,000 inhabitants by 2020 (CET, 2016). As part of the measures adopted for this project, the city implemented in 2015 a policy program reducing the speed limit in several major urban roads throughout the city. The program had two major phases. First in July 20, 2015, the speed limit of the main urban highways of the city (*Marginais*[3]) was reduced from 90km/h to 70km/h.[4] Then, in the following six months, the speed limit in the city arterial roads was reduced from 60km/h to 50km/h[5] (CET, 2016). By the end of this process, approximately 570 km of roads had their speed limit reduced.[6] Although these segments correspond to only 4% of the São Paulo road network, they account for about 29% of private vehicles VMT in the city.[7] Figure 1.1 maps the roads where speed limits were reduced, indicating the final speed limit in each treated road segment after the 2015 changes.

As shown in Figure 1.2, the implementation of lower speed limits was evenly distributed throughout the second semester of 2015. The figure plots the cumulative length of roads which were treated during the period, showing that the total length of treated segments increase almost constantly.

Moreover, besides the speed limit reductions, another major action of the road safety program was the expansion of speeding enforcement through the installation and upgrade of speed control cameras[8] (CET, 2016). Figure 1.3 plots the evolution in the number of speed control cameras in São Paulo between 2015 and 2016. The total number of cameras went from 381 in the beginning of 2015 to 698 by the end of that year, an increase of about 83%.

[3] The name "*Marginais*" comes from the location of these highways, which run through the banks (*margens* in Portuguese) of the *Pinheiros* and *Tietê* rivers.

[4] These values correspond to the speed limit reduction in the express lanes of the *Marginais*. In the intermediary and local lanes of the highways, speed limit was reduced respectively from 70km/h to 60km/h and from 70km/h to 50km/h.

[5] The only exceptions were the "*Corredor Norte-Sul*" (North-South Corridor), where the speed limit was reduced from 70km/h to 60km/h, and a few arterial roads where the speed limit was reduced to 40km/h.

[6] This figure does not weight the treated segment by their corresponding number of lanes.

[7] These figures were calculated by the authors by overlapping the simulated paths of trips reported in the 2012 Mobility Survey over the city's road network. VMT = Vehicle Miles Traveled.

[8] In Brazil, the main mechanism of speed limit enforcement are automated traffic cameras that register vehicle speed and issue speeding tickets to vehicles that are speeding. The fees for speed limit violations are defined by the National Traffic Code (*Código de Trânsito Brasileiro*). In 2015 the values for speeding were: a) R$85.13 for speeding up to 20% above the speed limit, b) R$127.69 if 20%-50% above the limit, c) R$574.63 if speeding above 50% the limit. In November 2016, these amounts were updated to respectively R$130.16, R$195.23 and R$884.41

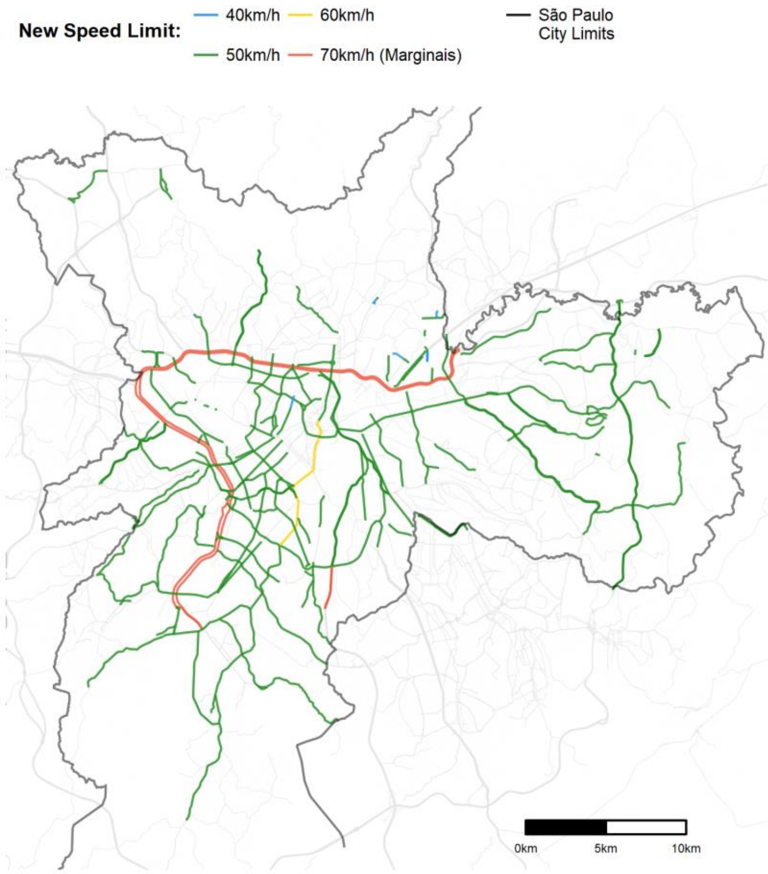

Figure 1.1: Road Segments with Speed Limit Reductions in 2015 by New Speed Limit

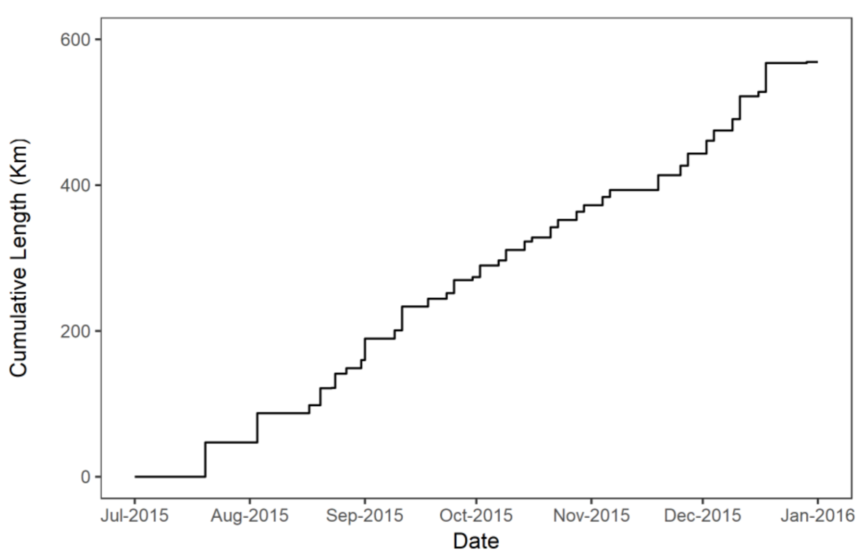

Figure 1.2: Cumulative Length of Road Segments with Speed Limit Reductions (July-December, 2015)

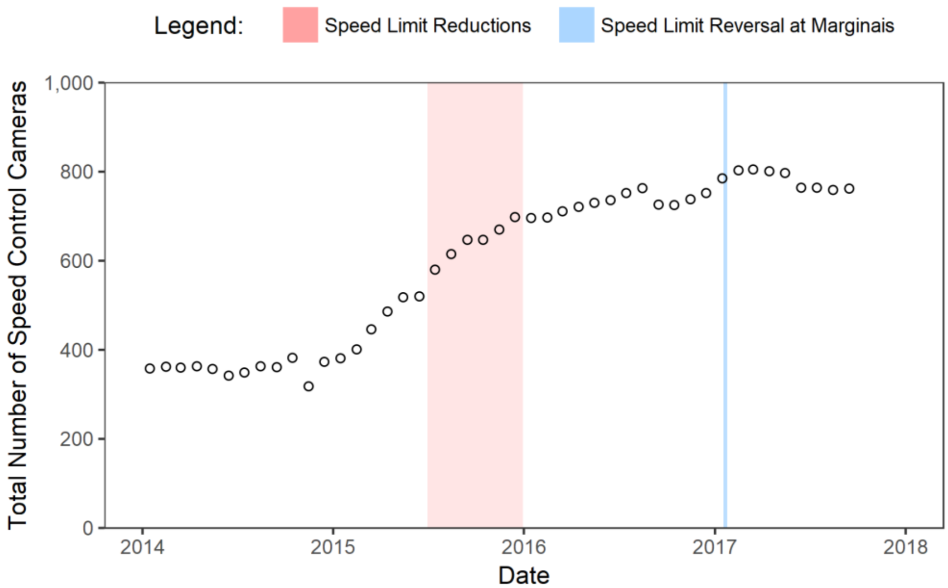

Figure 1.3: Number of Speed Control Cameras in São Paulo by Month (Jan/2015-Sep/2017)

Most of these new cameras were installed in the exact same road segments which had the speed limit reduced. Table 1.2 compares the location of cameras installed before and after the first speed limit reduction implemented on July 20, 2015, and Figure 1.4 maps the location of the new cameras installed after that same date. While 54% of cameras installed before July 20, 2015 were located on the road segments which would have the speed limit reduced, that share increased to 70% in the case of cameras installed after that date. Therefore, it is important to notice that along with the speed limit reduction, there was a concomitant expansion of speed limit enforcement, particularly on the roads where speed limit reduction was implemented.

1.1.2 *Marginais* Speed Limit Reversal in 2017

In January 2017, the speed limit in the *Marginais* was reverted to pre-2015 values, that is, it was raised back from 70 km/h to 90 km/h. This measure was adopted in the first month of the newly elected mayor term, and it was a major campaign proposal of the winning candidate during the electoral process. The debate about urban speed limits was a contentious topic during the

Table 1.2: Speed Control Cameras in São Paulo by Date of Installation and Treatment Group of their Location

	Installed before July 20, 2015		Installed after July 20, 2015	
	cameras	share	cameras	share
Treatment Group	434	0.54	210	0.70
Other Roads	369	0.46	92	0.30

Notes: Information about cameras and their location were extracted from the website *Painel Mobilidade Segura* (mobilidadesegura.prefeitura.sp.gov.br/) maintained by the São Paulo City Government. Road segments are defined as "Treatment Group" if they had their speed limit reduced in 2015 and "Other Roads" if no speed limit change occurred in that year.

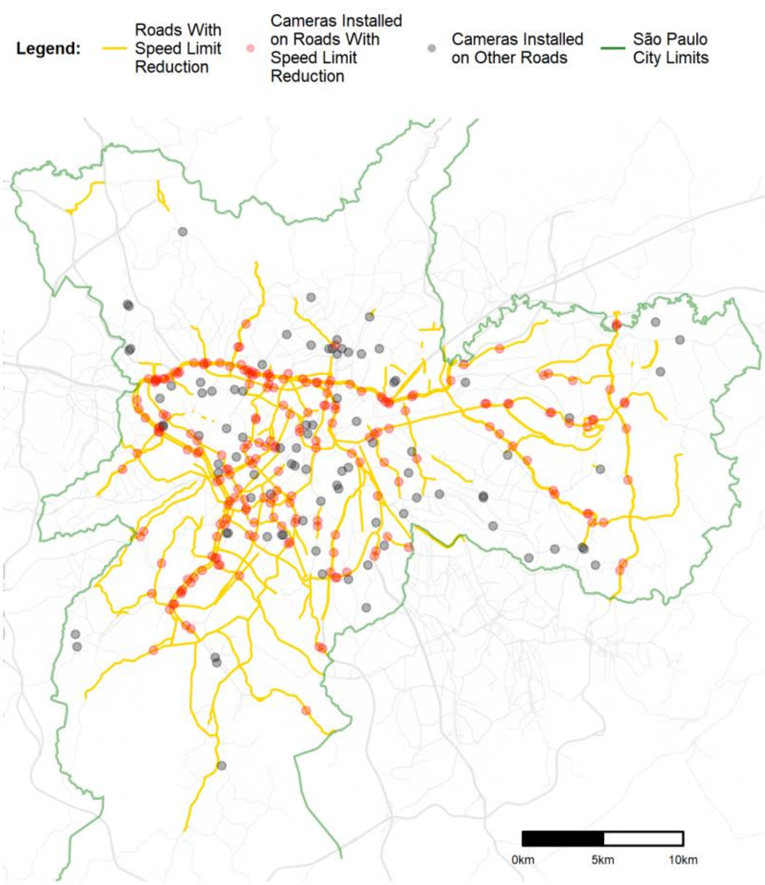

Figure 1.4: Speed Control Cameras Installed after July 20, 2015 and Road Segments with Speed Limit Reductions

political campaign, with candidates defending distinct policies on the matter.[9] However, contrarily to the speed reduction that took place in 2015, the speed limit reversal of 2017 was restricted to the *Marginais*, that is, the main urban highways of São Paulo. Meanwhile, the speed limit in the city arterial roads was not re-altered. Noteworthily, concomitant with the speed limit reversal of 2017, some compensating safety measure were adopted to attenuate the possible increase in accidents risk.[10]

Summing up the timeline of policies: throughout the second semester of 2015, the speed limits on urban highways and arterial roads were reduced due to a policy program aimed to reduce road accidents. In January of 2017, the reduction of 2015 was reverted on the main urban highways of the city. In the next session, we describe the datasets we use to evaluate the impacts of these policy changes.

1.2 Data

The main goal of this study is to assess the impacts of the speed limit changes adopted in the roads of São Paulo which were described in the previous session. While the speed limit reductions were aimed to decrease road accidents, the speed limit reversal was defended during the city electoral campaign as a measure to reduce commuting time. Therefore, we focus our analysis on these two outcomes, i.e., road accidents and commuting time. To estimate the policy impacts on these variables, we explore the following datasets:

a) Road segments with speed limit changes (2015 & 2017)
b) Traffic accidents (2012-2016)
c) Traffic control cameras and tickets (2014-2017)
d) Trip simulations scraped from a Web API (2016-2017)

[9] Some examples of the debate during the electoral campaign can be found at https://goo.gl/Ju38zV, https://goo.gl/LRFWsA and https://goo.gl/bXW82N.

[10] These measures included: the construction of elevated road steps on pedestrian crossing points at Marginais feeding lanes; placement of signs warning drivers about the presence of pedestrians; bus operators were instructed to provide additional safety courses to bus drivers; traffic agents with speed control pistols were placed throughout the *Marginais* (CET, 2016).

Next, we describe in further details each of these datasets and how we use them in our analyses. In the end of this session, we present a timeline that summarizes the policy changes evaluated in this paper and how our datasets overlap with them.

1.2.1 Road Segments with Speed Limit Changes

During the period when speed limit reductions were implemented in 2015, the traffic agency of São Paulo (CET) posted into its website a series of announcements indicating details about each upcoming speed limit changes. In these announcements, they described the exact road segments which would have the speed limit reduced and the date when the change would be implemented. These announcements also described the existing speed limit of these segments and the new limits that would be adopted.[11]

We collected all such reports from the agency website, and using their information, we geocoded the road segments where speed limit was altered, also associating the date when the new speed limit was adopted in each segment. On total, the 37 reports used in our study described the speed limit changes implemented on 202 different streets in São Paulo, and included approximately 570 km of treated road segments. Moreover, it is important to notice that before the actual speed limit reduction in each segment, the traffic agency of São Paulo would place a series of traffic signs and banners in those segments indicating the upcoming speed limit change.[12] This fact is important for our analysis because the simple presence of these signs could already have some effect on drivers' behavior.

1.2.2 Traffic Accidents

To evaluate the evolution of road accidents on treated segments, we analyze data from the São Paulo Traffic Agency yearly reports of road accidents from 2012 to 2016. These reports compile information for all road accidents registered within the city of São Paulo by the police

[11] Appendix A shows the information from one of these announcements and summary information for all 37 announcements used in this study.
[12] Appendix B shows an example of such signs.

departments, traffic agents', hospitals, and morgues (CET, 2017).[13] For each accident in these datasets we have: its location, time, number of victims, vehicles involved, the severity of injuries for each victim (unharmed, injured, dead), the alcohol level of drivers, victims age, gender and education attainment, and the type of vehicles involved in the accident (car, van, motorcycle, etc.). Table 1.3 presents some descriptive statistics of the accidents included in our dataset.

Moreover, we calculate the monetary cost of accidents using two sets of parameters. First, for vehicle costs and non-fatal victim's cost we use the parameters estimated by IPEA (2015). This study from the Brazilian Institute of Economic Research estimates the average cost of road accidents in Brazil by accident severity and status of victims. [14] Second, in the case of road fatalities, we use the Value of Statistical Life (VSL) of U$ 1.695 Million[15] estimated for Brazil by Viscusi & Masterman (2017). Given these parameters, Table 1.4 reports the descriptive statistics of the monetary costs of accidents included in our sample.

Table 1.3: Descriptive Statistics of Traffic Accidents in São Paulo (2012-2016)

	Total	2012	2013	2014	2015	2016
São Paulo						
Accidents	97,712	22,413	21,082	20,859	18,817	14,541
Injured Victims	114,527	26,933	24,875	24,381	21,656	16,682
Fatalities	4,127	907	822	938	780	680
Marginais [a]						
Accidents	3,026	633	639	767	613	374
Injured Victims	3,581	744	775	915	696	451
Fatalities	167	34	33	46	36	18

Notes: data from the São Paulo Traffic Agency (CET) reports of road accients. [a] Accidents in the Marginais include all data geocoded within 100m of the Marginais shapefile.

[13] We requested access to the individual accidents data from these reports using the Brazilian Law of Access to Information (*Lei de Acesso à Informação*). The requests for our project were registered as LAI request 21,151 opened in March 31, 2017 and LAI request 25,968 opened in November 1, 2017. As of the date of the latest version of this working paper, the report for 2017 have not yet been released.

[14] Appendix C presents all the parameters from Carvalho et al (2015) used in our study.

[15] In USD of 2015.

Table 1.4: Descriptive Statistics of Traffic Accident Costs in São Paulo

	Total	2012	2013	2014	2015	2016
São Paulo						
Total Cost (BRL million)[a]	30,119.0	6,788.4	6,197.7	6,711.3	5,694.0	4,727.5
Average Cost per Accident (BRL million)	0.308	0.303	0.294	0.322	0.303	0.325
Marginais [b]						
Total Cost (BRL million)	1,129.1	232.8	230.1	304.7	235.1	126.4
Average Cost per Accident (BRL million)	0.376	0.368	0.362	0.400	0.388	0.343

Notes: accident counts and accident characteristics were extracted from the datasets of the São Paulo Traffic Agency (CET) reports of road accients. Accident costs were calculated using the components estimated by Carvalho et al. (2015). In the case of road fatalities, the Value of Statistical Life of USD 1.69 Million estimated by Viscusi & Masterman (2017) for Brazil was used. [a] All values are presented in BRL December 2016 equivalents using the IPCA-IBGE inflation index. 1 BRL in December 2016 was equivalent to 0.31 USD. [b]Accidents in the Marginais include all data geocoded within 100m of the Marginais shapefile.

1.2.3 Electronic Traffic Tickets and Cameras

Unfortunately, there does not exist any readily available panel database of traffic volume per street in São Paulo. Therefore, to investigate the policy impacts on traffic volume, we explore data from the city automated traffic ticketing cameras. These devices automatically identify traffic violations issuing tickets that are mailed to the drivers' residences and are the main form of traffic enforcement in the City. The city government of São Paulo maintains a website called *Painel Mobilidade Segura*,[16] where data from these tickets is compiled. Available data about each ticket includes: the type of traffic violation that was registered, the date and hour of its occurrence, and its location. Table 1.5 summarizes the traffic tickets data we downloaded from the website. It corresponds to all electronic tickets issued in São Paulo between 2014 and September of 2017.

[16] "Safe mobility panel" in a direct translation from Portuguese.

Table 1.5: Summary Statistics of Electronic Driving Tickets issued in São Paulo (Jan/2014 – Sep/2017)

	Total			Tickets per Month (Million)			
	Tickets (Million)	Ticket Share	City Share	2014	2015	2016	2017[b]
São Paulo							
All Tickets	35.5	1.00	1.00	0.52	0.80	1.01	0.85
Driving Restriction	9.2	0.26	1.00	0.15	0.20	0.25	0.22
Speeding	19.2	0.54	1.00	0.26	0.43	0.56	0.47
Other	7.1	0.20	1.00	0.11	0.17	0.19	0.15
Marginais [a]							
All Tickets	9.64	1.00	0.27	0.12	0.21	0.32	0.20
Driving Restriction	2.13	0.22	0.23	0.03	0.05	0.06	0.05
Speeding	5.30	0.55	0.28	0.05	0.12	0.19	0.11
Other	2.21	0.23	0.31	0.04	0.05	0.06	0.03

Note: Data extracted from the São Palo Traffic Agency (CET) website *Painel de Mobilidade Segura* (http://mobilidadesegura.prefeitura.sp.gov.br). [a] Tickets in the Marginais include all data geocoded within 100m of the Marginais road shapefile. [b] Data for 2017 was only available up to September.

We use this dataset to construct a proxy of road usage over time. Specifically, we explore the number of driving restriction[17] tickets issued per road segment per day. To construct this proxy, we have to assume that, given a number of cameras, the number of driving restriction violations should be a direct function of the number of cars circulating on monitored road segments during restricted hours.[18]

17 São Paulo adopts a driving restriction scheme that limits the circulation of 20% vehicles at peak-hours in the central area of the city. The restriction is based on the final digit of vehicle's license plates. All Brazilian license plates have a number as their final digit, so the driving restriction of São Paulo limits the circulation of vehicles with license plates ending in 2 different numbers. For example, on Mondays, vehicles with final digits 1 and 2 are restricted to circulate in the São Paulo Downtown Area. Appendix D shows the Driving Restricti on Area of São Paulo and its overlap with the streets which had their speed limit altered in the period of our analysis.

18 We acknowledge that this measure is not a completely perfect proxy for traffic volume. First, we miss any information about non-restricted periods. Moreover, the measure is biased in the presence of unobserved factors that affect the number of tickets but may not be associated with traffic volume. However, we maintain it is a valid proxy for peak hours as it would identify non-marginal changes in road usage per segment in that period.

1.2.4 Travel Time

Travel time and vehicle speed databases with high temporal and spatial granularity are also not readily available for São Paulo. Therefore, in order to measure the effects of the speed limit changes on commuters' travel time, we explore the emergence and development of mobile web APIs that provide real time information about road traffic. Specifically, we simulated a set of representative motorized journeys using Google Directions API, which finds an optimal path for the provided pairs of origin and destination coordinates. Moreover, the API estimates travel time of simulated trips given real-time traffic conditions that are collected from drivers using Google Maps routing services and GPS information from mobile phones (Google, 2009).

The sample of representative journeys that we simulate was extracted from the São Paulo 2012 Mobility Study, a household travel survey that interviewed 8,115 households, collecting detailed information about 46,861 trips designed to be representative of commuting patterns in the whole Metropolitan Region of São Paulo in a regular weekday. Based on the start and end coordinates of motorized trips reported in this Survey, we ran a series of daily simulations using the web API. For each simulation, we collected the estimated travel time given real-time traffic conditions at the moment of each query. On total, we simulated 1.3 million trips between July 2016 to September 2017, hence covering the period when the speed limit was increased from 70km/h to 90km/h on the *Marginais* in January of 2017. Table 1.6 presents some descriptive characteristics of the simulated trips from our study.

1.2.5 Timeline of Policy Changes and Datasets

Figure 1.5 summarizes the timeline of policy changes we evaluate in this paper and each of the datasets we use in our analyses. While our dataset of road accidents overlaps the period from before and after the speed limit reductions of 2015, we do not have information about road accidents after the speed limit increase of 2017. On contrary, while we do not have trip simulations from before the speed limit reductions of 2015 we do cover the before-and-after period in the case of 2017. Therefore, while in the case of accidents we can only evaluate the impact of the 2015

Table 1.6: Summary Statistics of Trips Simulated Using Google Directions API (Jul/2017 – Sep/2018)

	All Crawled Trips				Trips over Marginais[3]			
	Obs. (Thousand)	Share	mean	s.d.	Obs. (Thousand)	Share	mean	s.d.
Crawled Trips[1]	1,471.6	1.00			243.7	1.00		
Post Speed Limit Increase[2]	511.5	0.35			84.8	0.35		
Peak	562.2	0.38			96.8	0.40		
Rain	93.6	0.06			15.6	0.06		
Use Marginais[3]	243.7	0.17			243.7	1.00		
Travel Time[4] (Minutes)			20.00	17.11			38.42	19.83
Travel Length[4] (km)			8.31	9.18			18.49	12.24
Ratio at Marginais[3]			0.04	0.12			0.22	0.21

Notes: [1] private vehicle trips reported on the 2012 São Paulo Mobility Household Surey were crawled on Google Directions API using their origin and destination coordinates between July 4, 2016 and September 1, 2017. [2] The speed limit increase in the Marginais was implemented in January 25, 2017. [3] We identify trips over the Marginais by comparing the intercect between the optimal path suggested by OSRM API and a buffer of 200m around the Marginais shapefile. All trips with more than 400m of intercection are defined as running through the Marginais. [4] Travel time and distance are reported by Google Directions API for each query.

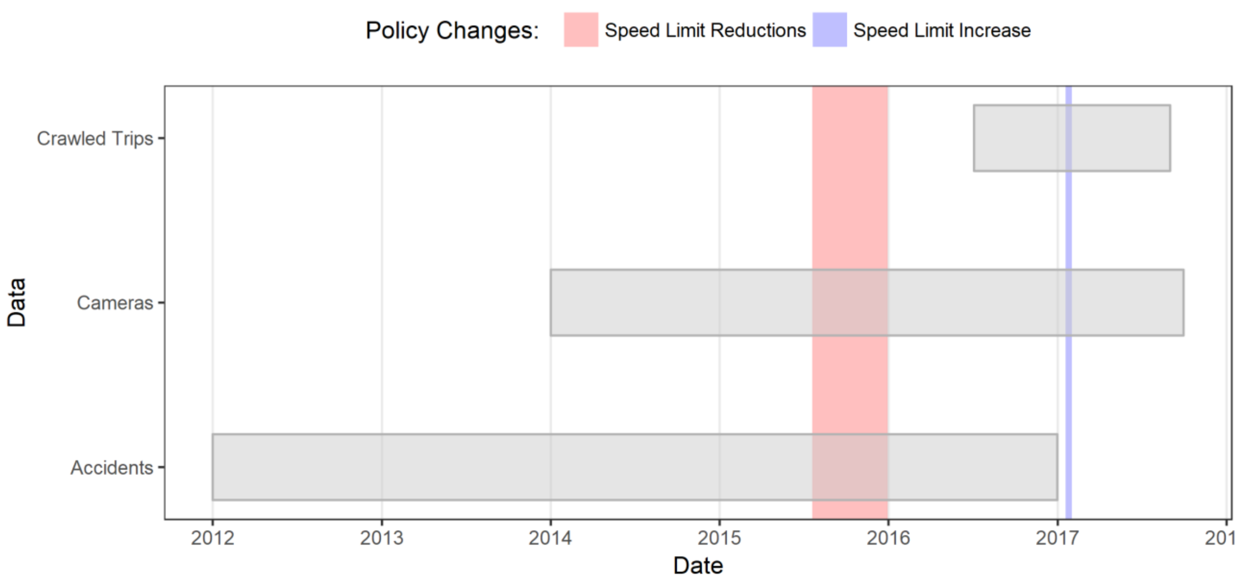

Figure 1.5: Timeline of Speed Limit Changes and Datasets used in our Analyses

speed limit reductions, in the case of travel time we are limited to the 2017 speed limit increase. For both policies, we have information from driving restriction tickets, so we are able to evaluate their impacts on road volume.

1.3 Empirical Model and Reduced Form Results

In this session we describe our baseline empirical models used to estimate the impacts of the speed limit changes in São Paulo. First, we investigate the effect of the 2015 speed limit reduction on road accident, and then, we explore our dataset of crawled trips to estimate the effect of the speed limit increase of 2017 on commuters' travel time.

1.3.1 The Effects of the 2015 Speed Limit Reductions on Road Accidents

To identify the effect of the 2015 speed limit reductions on road accidents, we explore the time and geographical heterogeneity of speed limit reductions on different roads in São Paulo. We start with a static model that assumes a single and constant treatment effect. We then extend our estimation using a dynamic model that allows for the treatment effect to vary over time.

All models are estimated using a panel of 1,578 road segments with 400m of length each. These segments compose all roads where the speed limit was reduced in 2015 and where at least one accident was reported in the period covered by our analysis.

For each segment i, we identified the speed limit reduction date t_i^0 and all corresponding road accidents from 2012 to 2016. We start our analysis by defining the "relative time distance" d of each panel observation to its corresponding speed limit reduction date t_i^0:

$$d = t - t_i^0 \qquad (1)$$

Where t is the calendar date of each panel observation, so d indicates how far in time each panel observation is from its corresponding change date t_i^0. We then aggregate the panel in time using bins of seven "relative days" so we avoid composition effects due to weekday heterogeneities. We define these aggregated time periods as "relative weeks" w:

$$w = \left\lfloor \frac{d}{7} \right\rfloor \tag{2}$$

From the relative distance of each observation to the treatment date, we have a direct correspondence to its treatment status T_{iw}, that is:

$$\begin{aligned} T_{iw} &= 1 \qquad if \qquad w \geq 0 \\ T_{iw} &= 0 \qquad if \qquad w < 0 \end{aligned} \tag{3}$$

Within this setup, each road segment before the speed limit reduction can be understood as a control observation for itself after the new speed limit is adopted. Figure 1.6 illustrates our empirical design by plotting the average number of accidents and accident costs per segment per week by their relative distance to the speed limit reduction date.

To calculate the average effect of the speed limit reduction on road damages, we estimate the following empirical model:

$$y_{iw} = \mu_i + \beta T_{iw} + \gamma T_{iw} C_{iw} + \delta(1 - T_{iw}) C_{iw} + \gamma X_w + \varepsilon_{iw} \tag{4}$$

Where y_{iw} can be any of the following variables: road accidents, road injuries, road fatalities, or total accident cost. Moreover, in each estimation the dependent variables are divided by their corresponding average by road type in the pre-treatment period, so the treatment effect is directly estimated as an average relative change.[19]

[19] To adjust for segment heterogeneities and avoid overweighing observations with very few accidents, we weight each observation in the regressions by the total accident damages per segment in the pre-period.

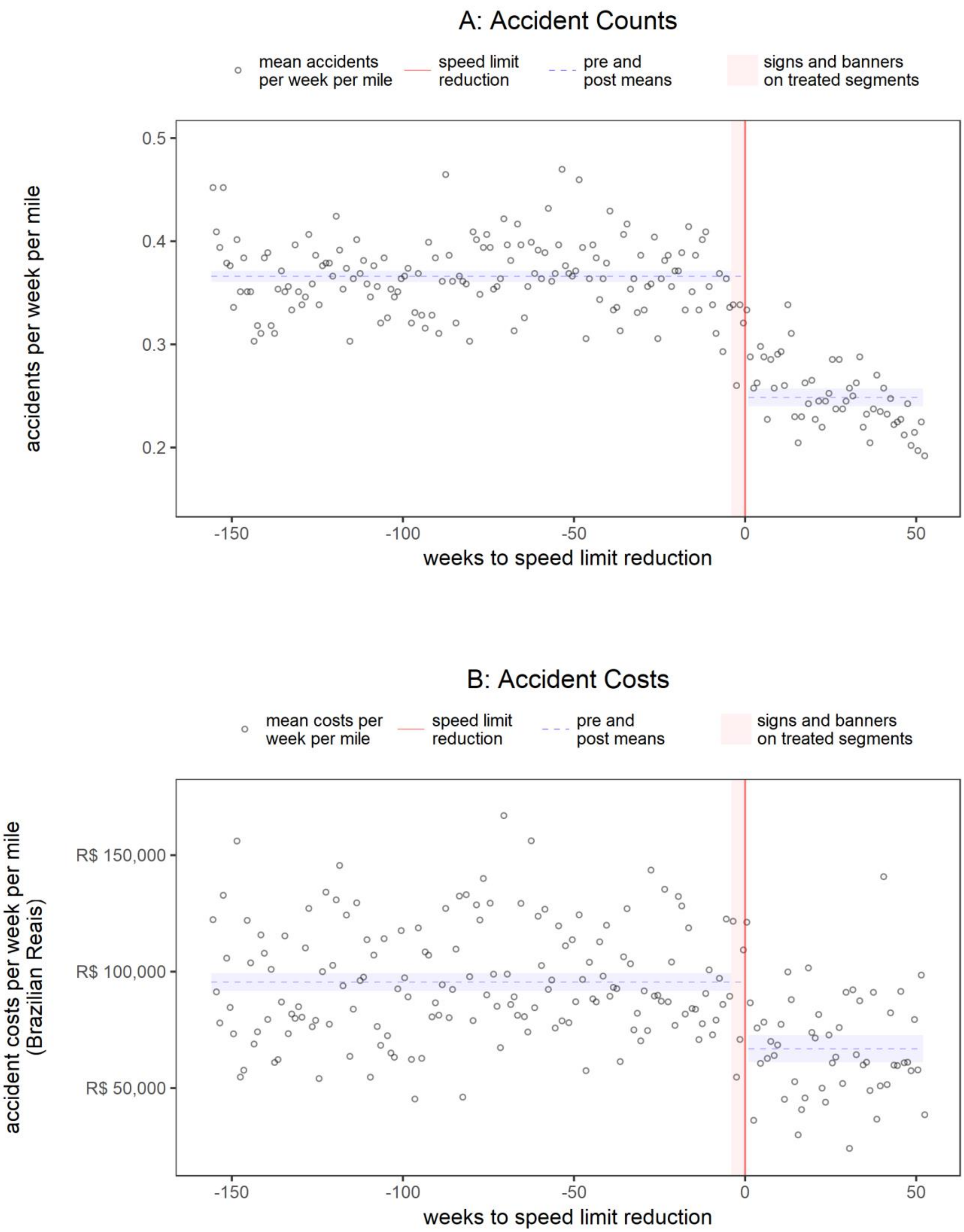

Figure 1.6: Average Number and Cost of Road Accidents per Segment per Week by Relative Time Distance to Speed Limit Reduction

As for the explanatory variables, μ_i is a road segment fixed effect that accounts for time invariant characteristics of segments. As described before, T_{iw} is an indicator if the speed limit was already reduced in segment i on week w, so β indicates the average effect of the policy net of camera effects and which is our main result of interest. Additionally, C_{iw} indicates the presence of an active speed monitoring camera on segment i during week w. Therefore, while coefficient γ can be interpreted as the effect of having a camera on a segment before the speed limit change, δ indicates the similar effect after the policy change. The difference between δ and γ indicate the interaction effect between the speed limit reduction and the presence of camera enforcement. Finally, X_w is a vector of citywide time varying covariates. It includes fuel sales, and calendar month fixed effect which account for within year seasonality.

Additionally, to avoid possibly confounding effects caused by the placement of traffic signs and banners on treated roads in the weeks preceding the speed limit changes, in our baseline specification, we exclude observations from the four weeks immediately before the speed limit reduction in each segment.

On Table 7 we present the estimation results of our empirical model using accident counts as our dependent variable. We explore different model specifications, starting with Model 1 which does not account for any type of effect from speed control cameras. In this case, the results indicate an average reduction in accident of 36% after the speed limit reduction. We then add the camera covariate C_{iw}on Model 2, and the results indicate that the presence of speed control cameras are associated with an average decrease of approximately 7.1% of accidents on the street segments where these cameras are present. The inclusion of cameras as a covariate slightly decrease the point estimate for the speed limit reduction effect by about 0.9%. Then, on Model 3, we separate the effect of cameras into two parts: 1) the effect of cameras before the speed limit reduction, and 2) the effect of cameras after the speed limit reduction. The results of this final specification show that the effect of cameras is significantly larger after the speed limit changes, indicating an important interaction effect between the policies. Although the coefficient for the effect of cameras before the reduction is also negative, it is not significant, and the magnitude of the point estimate is about half of the effect of cameras after the speed limit reduction. Overall, the results from these estimations indicate that even by accounting for the effect speed camera enforcement, the speed limit reduction is still responsible for an average decrease of approximately 34.8% in traffic accidents on treated segments.

Next, on Table 8, we present the results from our preferred specification for the different dependent variables included in our analysis. The first line of results corresponds to the pooled sample where we include all treated road segments. The second and third rows show the results for the estimation of the model where we subset the data to respectively the *Marginais* and arterial roads.

Table 1.7: Regression Results: Changes in Road Accidents After Speed Limit Reductions of 2015

	Changes in Accident Counts (BRL)		
	(1)	(2)	(3)
Post SLR[a]	-0.362 ***	-0.353 ***	-0.348 ***
	(0.015)	(0.016)	(0.017)
Camera × Post SLR			-0.076 ***
			(0.022)
Camera × Before SLR			-0.043
			(0.037)
Camera		-0.062 **	
		(0.024)	
Fuel	0.170	0.206	0.195
	(0.154)	(0.152)	(0.150)
Segment FE	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Obs.	397,650	397,650	397,650

Notes: * p < 0.05, ** p < 0.01, * p < 0.001. Coefficients indicate the average changes of dependent variables with respect to pre-treatment means. For example, a coefficient of -0.5 indicates a reduction of 50%. Standard Errors are clustered by Street (202 clusters). Covariates used in the model include: fuel sales, calendar month fixed effects and a linear time trend.

Table 1.8: Regression Results: Changes in Road Accidents, Fatalities and Injuries after Speed Limit Reductions of 2015 by Road Type

	Accidents	Injuries	Fatalities	Costs
All Roads	-0.348 ***	-0.343 ***	-0.214 **	-0.275 ***
	(0.017)	(0.023)	(0.070)	(0.043)
Marginais	-0.472 ***	-0.451 ***	-0.381 *	-0.449 ***
	(0.046)	(0.058)	(0.193)	(0.121)
Arterial Roads	-0.337 ***	-0.333 ***	-0.198 *	-0.259 ***
	(0.016)	(0.025)	(0.077)	(0.046)

Notes: * p < 0.05, ** p < 0.01, * p < 0.001. Coefficients indicate the average changes of dependent variables with respect to pre-treatment means. For example, a coefficient of -0.5 indicates a reduction of 50%. Standard Errors are clustered by Street (202 clusters). Covariates used in the model include: fuel sales, calendar month fixed effects and a linear time trend.

The point estimates indicate substantial decreases of road accidents on treated road segments. As we had already seen from Table 1.7, the overall average reduction on accidents was of 34.8%. Moreover, fatalities fell by 21.4%, road injuries decreased by 34.3% and accident costs by 27.5%. When breaking down the results by road type, we observe that the estimated effects of the policy on the *Marginais* were larger for all variables, although the point estimates are less precise because of the lower count of events, particularly in the case of fatalities. Meanwhile, the average policy effects on other arterial roads were roughly the same as in the pooled model, although slightly smaller in magnitude.

These results assume a static treatment effect; however, the impacts of the speed limit reductions may present a dynamic pattern, with increasing or decreasing effects over time. To investigate such possibility, we also estimate an extended version of our model where we break or treatment indicator T_{iw} by "relative quarters" after the speed limit reduction in each segment.

$$y_{iw} = \mu_i + \sum_{Q} \beta_Q T_{Qiw} + \gamma X_w + \varepsilon_{iw} \tag{5}$$

Figure 1.7 mirrors Table 1.8 by plotting the coefficients we estimate for each of our dependent variables and subsets of road types from the dynamic event study model. The results indicate that, overall, policy effects are increasing over time, with fewer road accidents and injuries per segments in the post-treatment treatment panel observations further away from the speed limit reduction in each segment. However, in the case of road fatalities, and consequently accident costs, estimates are not sufficiently precise for a precise inference of dynamic patterns.

1.3.1.1 Possibility Confounding Factors – Pre-treatment trends

One underlying assumption required for the internal validity of our event study estimates is the absence of unobserved pre-treatment trends in the dependent variables. To test for the validity of this assumption, we run the pair of tests proposed by Borusyak & Jaravel (2016). First, we estimate an extended version of our dynamic model where we include pre-treatment relative quarter fixed effect terms. We then run an F-test comparing the significance of including these coefficients in the estimation. Figure 1.8 shows the plot of pre-treatment coefficients, indicating that almost none of the point estimates are individually significant. Moreover, Table 1.9 shows the F-test for the statistical significance of pre-treatment coefficients as a group for each of our specifications. The results indicate that the pre-treatment coefficients are mostly not significant, although some of the statistic are near the limits of 5% significance. Therefore, these results indicate that although we cannot completely rule out unobserved pre-treatment trends in our dependent variables, the confounding effects of any existing trend is unlikely to affect meaningfully our main estimates.

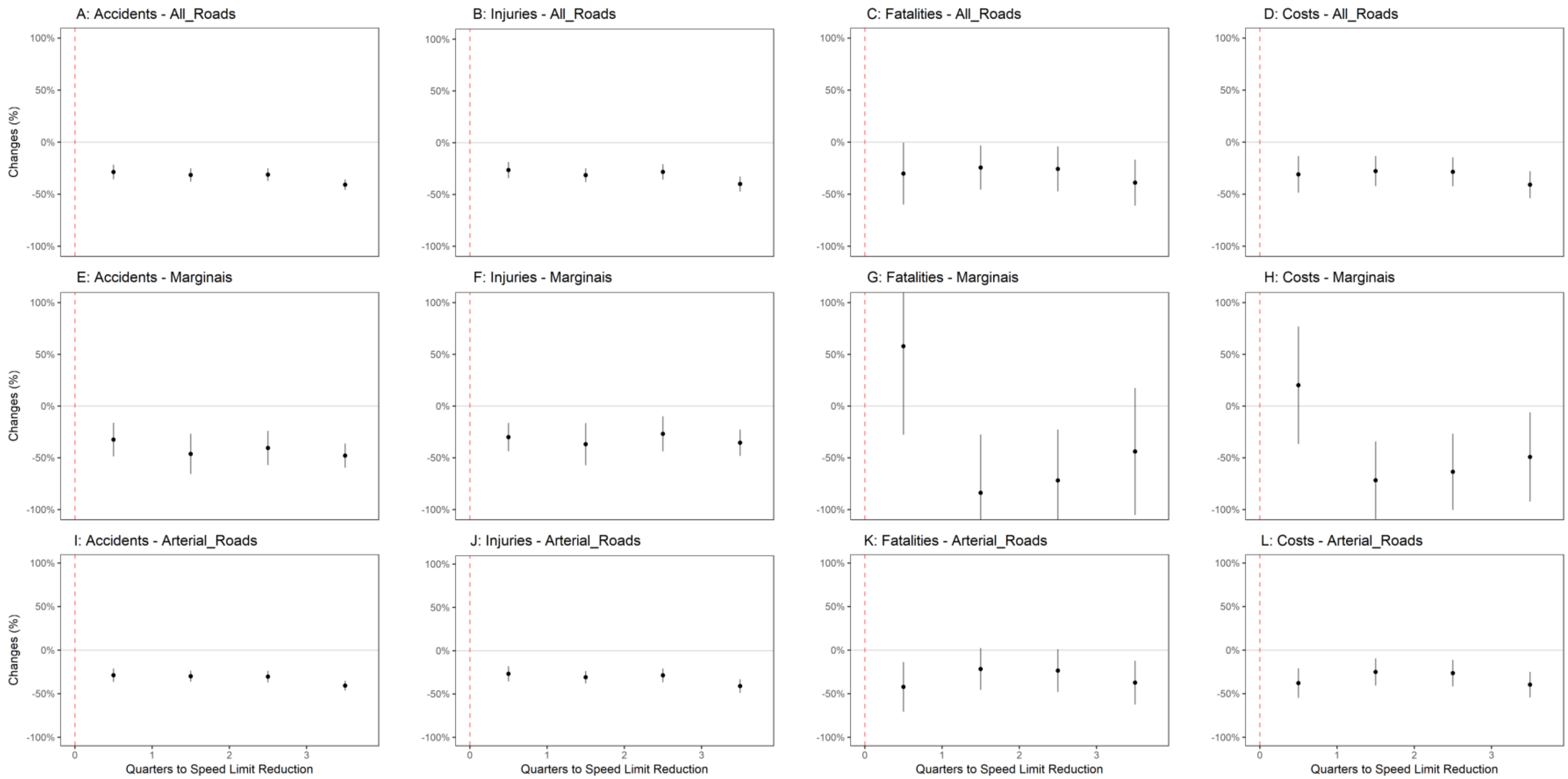

Figure 1.7: Event-Study Results: Treatment Effect by Quarter After Speed Limit Reduction

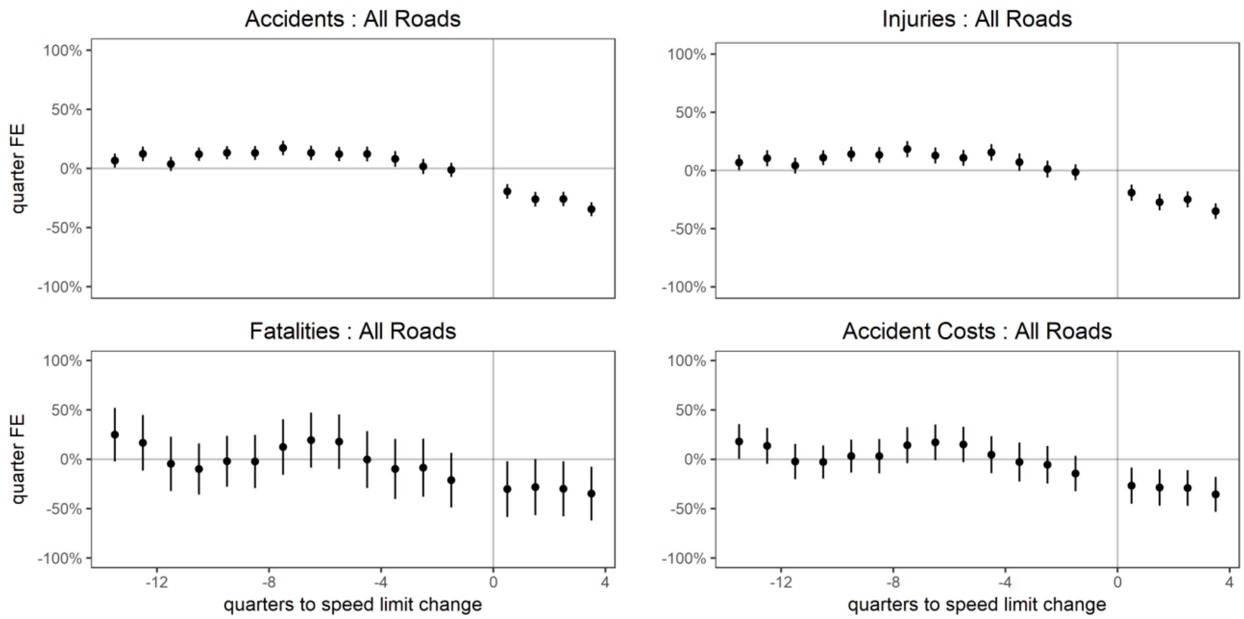

Figure 1.8: Regression Results: Pre-Treatment Relative Quarters Fixed Effects

Table 1.9: F-Test statistics for the inclusion of pre-treatment quarter specific fixed effects

	Accidents	Injuries	Fatalities	Costs
All Roads	1.806 *	1.694	0.765	1.050
Marginais	0.622	1.514	0.937	1.100
Arterial Roads	1.714	1.763 *	1.306	1.474

1.3.1.2 Possibility Confounding Factors – Road Substitution

An additional concern for the validity of our estimates as the causal effect of the speed limit reductions is that a possible unintended consequence of the policy is the diversion of drivers to other routes. If that was the case, at least part of the observed reduction in accidents could be caused not by an actual reduction in accident risk, but by the lower traffic volume on treated roads.

To check if there was any road substitution associated with the speed limit reductions, we run two empirical tests. First, we evaluate if the share of driving restriction tickets issued on treated road segments decreased after the speed limit reductions. Second, we analyze the changes in road accidents on non-treated roads. The intuition behind the first experiment is that if the number of vehicles using treated roads had decreased after the speed limit reduction, then we should observe a proportional decrease in the number of driving restriction tickets issued on those same roads. In the case of the second exercise, the idea is that if drivers substituted away from treated roads, and that led to a reduction in accidents in those roads, then, through the same mechanism, we should observe an increase in accidents on the roads where these drivers substituted to.

In the case of driving restriction tickets, we first show the overlap between the driving restriction area of São Paulo and the roads with speed limit reductions.[20] On total, about 38.3% of treated road segments are within the driving restriction area. Moreover, out of the 225.4 thousand driving restriction tickets in our sample, 65.5% were registered on the roads which had their speed limit reduced in 2015. Therefore, it is likely that if the number of cars circulating on these roads had decreased after the speed limit reductions, then we should be able to observe some effect on the share[21] of tickets in these same roads.

To test if the speed limit reductions led to a decrease in the share of driving restriction tickets on treated road segments, we estimate the following model:

---

[20] Appendix D maps the driving restriction area and the set of roads with speed limit reductions within it.

[21] We use shares instead of counts because, as described earlier in the background session, there was a large expansion in the number of traffic monitoring cameras in the period of our analysis. So, the count of tickets would be confounded by the effect of camera expansion.

$$SDRT_{it} = \alpha_i + \beta T_{it} + \varepsilon_{it} \tag{6}$$

Where $SDRT_{id}$ is the share of driving restriction tickets issued on road i and date t. Like the accidents model, T_{it} is the speed limit reduction status of road segment i on day t. The coefficient α_i is a road segment fixed effect that controls for the average share of tickets issued on each road segment in the whole period. We also estimate an extended version of the above model where we split the treatment effect by quarters after the policy change to investigate possible heterogeneities of the effect of interest on time. Table 1.10 presents the results of these estimations. The results indicates that the share of driving restriction tickets on treated road segments have not changed significantly after the speed limit reduction. Overall the share of tickets increased by 0.5% after the policy, however such result is not statistically significant. Moreover, no significant pattern of any kind is observed from the dynamic model estimates.

Table 1.10: Regression Results: Changes in the Share of Driving Restriction Tickets after Speed Limit Reductions

	Changes in the Share of Driving Restriction Tickets	
PSLR[a]	0.005	
	(0.022)	
PSLR % 1st Quarter		-0.007
		(0.048)
PSLR % 2nd Quarter		-0.039
		(0.054)
PSLR % 3rd Quarter		0.005
		(0.044)
PSLR % 4th Quarter		-0.003
		(0.051)
Road FE:	yes	yes

Notes: * $p < 0.05$, ** $p < 0.01$, * $p < 0.001$. Coefficients indicate changes relative to the average before the speed limit reduction. A coefficient of -0.5 indicates a reduction of 50%. [a] PSLR = Post Speed Limit Reduction

The point estimates of this regression are small and far from any statistical significance, indicating no evidence of any change in traffic volume associated with the speed limit reductions.

However, it is important to acknowledge that using driving restriction tickets as a proxy for traffic volume have two main limitations: 1) the share of driving restriction tickets on treated segments may also be affected by the installation of new cameras on treated segments, so an eventual reduction in volume could counterweighted by the effect of additional cameras; 2) driving restriction tickets are constrained to peak hours, hence not fully reflecting all possible substitution patterns that could occur throughout the day.

Given these limitations, we run an additional test for road substitution where we investigate the changes in road accidents on non-treated road segments in the city. If diversion of traffic was responsible for the reduction of accidents on treated roads, then the reflexive effect would be an increase of accidents on other roads. However, as seen in Figure 1.9, the total number of road accidents on non-treated roads does not seem to have increased after the speed limit reductions of 2015.

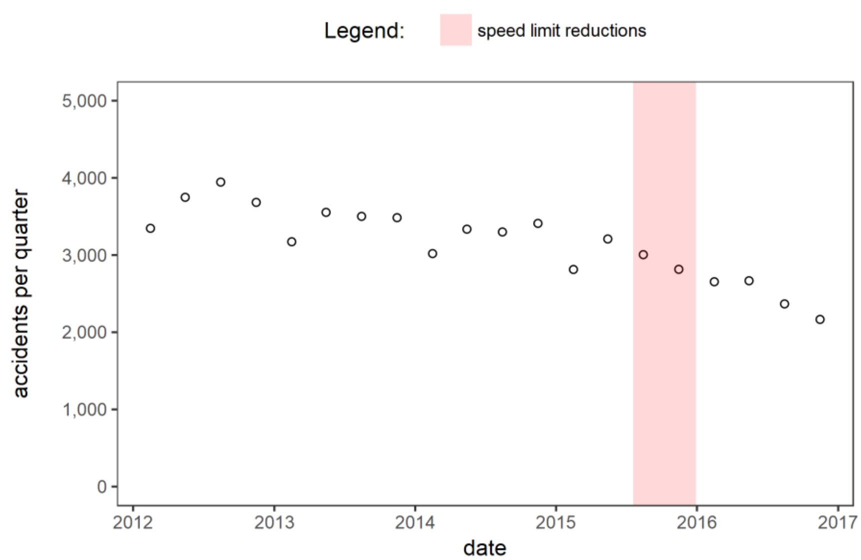

Figure 1.9: Road Accident on Roads Without Speed Limit Reductions per Quarter

Still, it could be the case that traffic from treated segments had moved to some specific road segments in the city, so the increase in accidents would not be reflected on city aggregates. Unfortunately, there is no trivial way to identify the most likely routes where drivers would substitute to. However, since treated roads were composed by the city main arterial roads and highways, it is not unreasonable to assume that any substitution would be concentrated on the major roads segments from the non-treated group. Therefore, to narrow our investigation of any potential substitution caused by the speed limit reduction, we focus our analysis on the larger road segments that were not affected by the speed limit reductions. To identify such segments, we use the number of road accidents observed per segment before July 2015, the date of the first speed limit reduction. We select all segments with 12 or more accidents in that period, a value that corresponds to the median among all non-treated segments. Figure 1.10 shows the segments selected through this procedure, showing that they roughly delineate some specific roads which were not affected by the speed limit reduction policy.

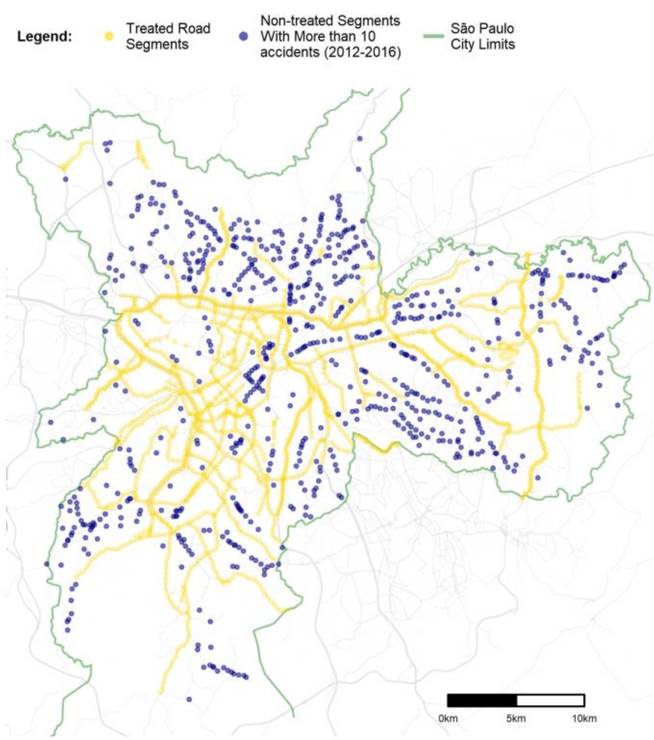

Figure 1.10: Non-Treated Road Segments with 12 or More Road Accidents Between Jan/2012-June/2015

We then estimate Equations 4 and 5 using these segments. Because these segments do not have a speed limit reduction date associated with them, we define July 20, 2015 as a placebo treatment date for all segments. The results from these estimations are presented in Table 1.11 and Figure 1.11, and they indicate negligible effects of the speed limit reductions on accidents on non-treated roads. If anything, although statistically non-significant, the point estimates indicate a small reduction of accidents on non-treated segments.

Table 1.11: Regression Results: Changes in Road Accidents, Fatalities and Injuries on Non-Treated Road Segments after July 20, 2015

	Accidents	Injuries	Fatalities	Costs
Non Treated Roads	-0.061	-0.072	-0.154	-0.125
	(0.037)	(0.044)	(0.168)	(0.108)

Notes: * $p < 0.05$, ** $p < 0.01$, * $p < 0.001$. Coefficients indicate the average changes of dependent variables with respect to pre-treatment means. For example, a coefficient of -0.5 indicates a reduction of 50%. Standard Errors are clustered by Street (202 clusters). Covariates used in the model include: fuel sales, calendar month fixed effects and a linear time trend.

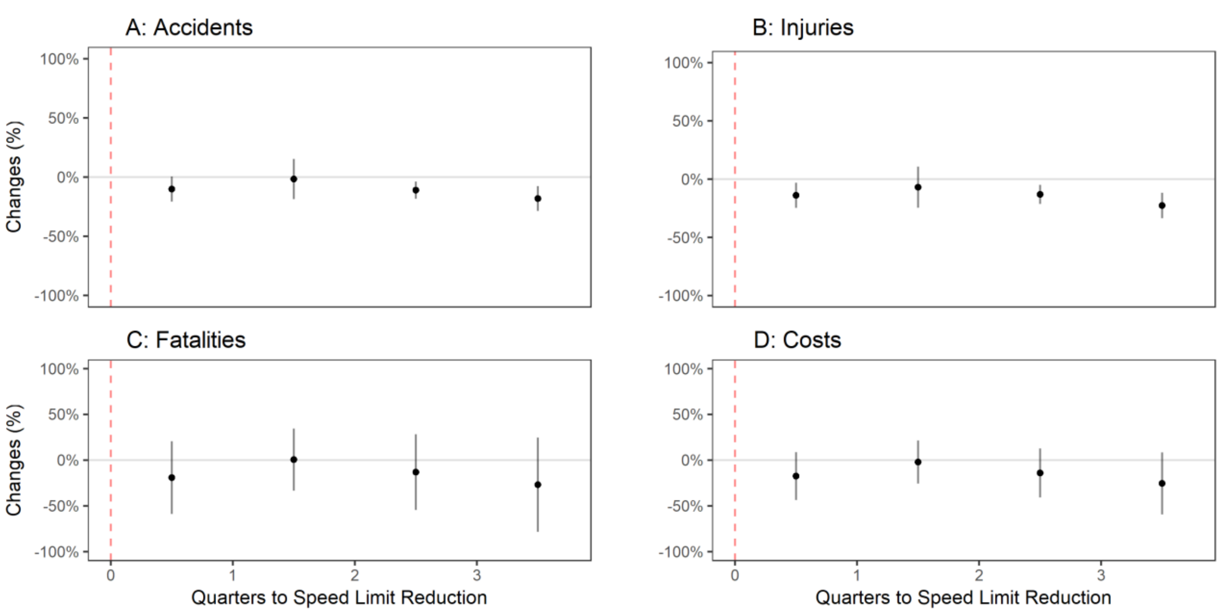

Figure 1.11: Event-Study Results: Placebo Treatment Effects on Non-Treated Roads by Quarters After First Speed Limit Reduction

Summing up, given that we do not observe any changes in the share of driving restriction tickets on treated segments, and no increases of accidents on non-treated segments, we are led to the conclusion that road substitution is unlikely to have played any significant role in explaining the reduction in road accidents observed after the speed limit reductions. Hence, these results corroborate the validity of our main estimates as the causal effect of the speed limit reduction on the the decrease of accients on treated segments.

1.3.2 The effect of the 2017 Speed Limit Increase on Commuting Time

We now analyze the effect of the speed limit increase January 2017 on the travel time of drivers in São Paulo. Using the dataset of trips simulated on Google Directions API, we compare the estimated travel time of trips before and after the policy change. Our baseline empirical strategy can be described by the following equation:

$$ETT_{ihd} = \alpha_{ih} + \beta Ma_i I_d + \gamma I_d + \delta X_{hd} + \sum_L \phi_L B_{Li} I_d + \varepsilon_{ihd} \tag{8}$$

Where ETT_{ihd} is the log of estimated travel time for each simulated trip i queried at hour h on date d. As for the explanatory variables, α_{ih} is a trip-hour fixed effect that controls for trips specific characteristics such as length, path and departure time.[22] Moreover, Ma_i indicates the share of each trip that takes place on the *Marginais,* and I_d indicates if the query was made after the speed limit increase in January 25, 2017. Therefore, while the coefficient γ captures the average change on estimated travel time for all trips, the coefficient β indicates the additional travel time change that was specific for trips using the *Marginais* proportional to the share of trips made on those highways. Moreover, X_{hd} is a vector of covariates that include the occurrence of rain in the moment of the query and if date d was a holiday in São Paulo.

Additionally, due to the interconnective of congestion effects within the road network, it is possible that the effects of the speed limit reduction may not be constrained to trips taken in the

[22] In this model, a trip is defined as a pair of origin and destination coordinates queried at a certain hour of the day. Therefore, a query that simulates traveling from point A to point B at 7am is considered as a different "trip" from the exact same query made at 8am.

Marginais. Instead, travel time reductions could also occur on nearby roads due to spillover effects. To account for these possible effects, we include in our specification a set of terms that identify the ratio of trips that take place within buffers of certain distances L from the *Marginais*. In our baseline specification, we include three levels of non-overlapping buffers with respectively 1km, 3km and 5km. Figure 1.12 illustrates the areas created through this process.

Table 1.12 shows the results from our regressions. The first column corresponds to the simplest version of our empirical model, where we do not include the spillover area terms. In this case, the main estimate of interest, that is the interaction between the speed limit increase indicator and the ratio of trips on the Marginais indicate a reduction of 6.5% on the estimated travel time for a trip completely made at the *Marginais*. This effect is already accounting for an overall reduction

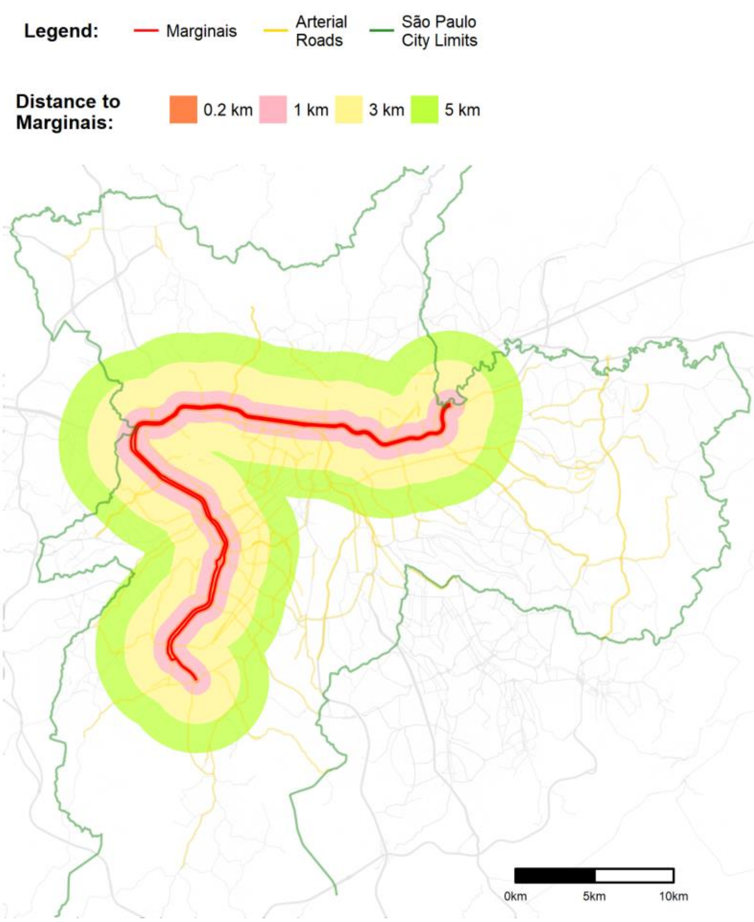

Figure 1.12: Marginais and Possible Spillover Areas

of 1.8% in travel time for all trips in our sample, irrespectively if they used or not the *Marginais*. Moreover, the coefficients associated with our additional covariates indicate that while rain led to an average increase of 1.9% in travel time, on holidays trips were on average 10% faster.

Table 1.12: Regression Results: Changes in Travel Time After the Marginais Speed Limit Increase of January 25, 2017

	Changes in Estimated Travel Time			
	(1)	(2)	(3)	(4)
Post SLI - Ratio at Marg.	-0.065 ***	-0.058 ***	-0.057 ***	
	(0.014)	(0.013)	(0.013)	
Post SLI - Ratio at Marg. - Peak				-0.027
				(0.025)
Post SLI - Ratio at Marg. - OffPeak				-0.075 ***
				(0.011)
Post SLI	-0.018 ***	-0.011 ***		
	(0.004)	(0.003)		
Post SLI - Ratio at 1km of Marg.		-0.034 ***	-0.035 ***	-0.034 ***
		(0.008)	(0.008)	(0.008)
Post SLI - Ratio at 3km of Marg.		-0.016 **	-0.015 **	-0.015 **
		(0.005)	(0.005)	(0.005)
Post SLI - Ratio at 5km of Marg.		-0.009	-0.008	-0.008
		(0.004)	(0.005)	(0.005)
Rain	0.019 ***	0.019 ***	0.022 ***	0.022 ***
	(0.005)	(0.005)	(0.005)	(0.005)
Holiday	-0.100 ***	-0.100 ***	-0.096 ***	-0.096 ***
	(0.009)	(0.009)	(0.010)	(0.010)
Trip-Hour FE	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	Yes
Obs.	1,337,555	1,337,555	1,337,555	1,337,555

Notes: * p < 0.05, ** p < 0.01, * p < 0.001. Coefficients indicate the average changes of dependent variables with respect to pre-treatment means. For example, a coefficient of -0.5 indicates a reduction of 50%. Standard Errors are clustered by date Street (191 clusters). Post SLI is a dummy that indicates queries made after the speed limit increase on the Marg. in January 25, 2017. Rain is a dummy indicating if there was positive registers of rain during the hour of each query was made. Trip-Hour Fixed effects include a specific intercept for each pair of origin and destination coordinates queried in a certain hour of the day.

Next on column 2 we include the covariates associated with the portion of trips that take place on the spillover areas. The inclusion of these variables reduces the main treatment effect to 5.8%. However, for the portion of trips taking place within 1 km of the Marginais, the results indicate an average estimated travel time decrease of 3.4%, or approximately 60% of the main effect observed on the Marginais. Similarly, the estimated spillover effects on the 3 km and 5 km buffers were of respectively 27% and 15% of the main effect, although the later was not statistically significant. On column 3, we include month specific fixed effects, however that doesn't affect any of the estimates significantly.

Finally, on Column 4 we separate the main treatment effect into two parts, one associated with trips taken during peak hours, and other for trips in the off-peak. The results from this split indicate that most of the travel time reduction in the *Marginais* occurred in off-peak hours, when the corresponding point estimate indicate an average reduction of 7.5%. meanwhile, for the peak period, the average reduction was of only 2.7% and the point estimate for this coefficient is not statistically significant.

Lastly, similar to the case of our empirical model for accidents, we estimate an event-studies version of our model where we interact our main treatment effect component with each quarter after the policy change. The resulting estimates associated with the share of trips in the *Marginais* and spillover areas are presented on Figure 1.13. It indicates that the treatment effect of the policy seems to be increasing on time, with larger travel time reduction for trips further away in time to the policy change. Meanwhile, the spillover effects do not seem to present any clear dynamic pattern over time.

Summing up the results of our empirical models, we have found that the speed limit reductions of 2015 were associated with a reduction of approximately 34.8% on road accidents and 21.4% of road fatalities on treated segments. Moreover, we did not find any evidence of traffic volume reduction associated with the policy. Meanwhile, after the speed limit increase of 2017, the portion of trips that take place on the Marginais became approximately 6.5% faster. In the next session, we compare the magnitude of these results in economic terms.

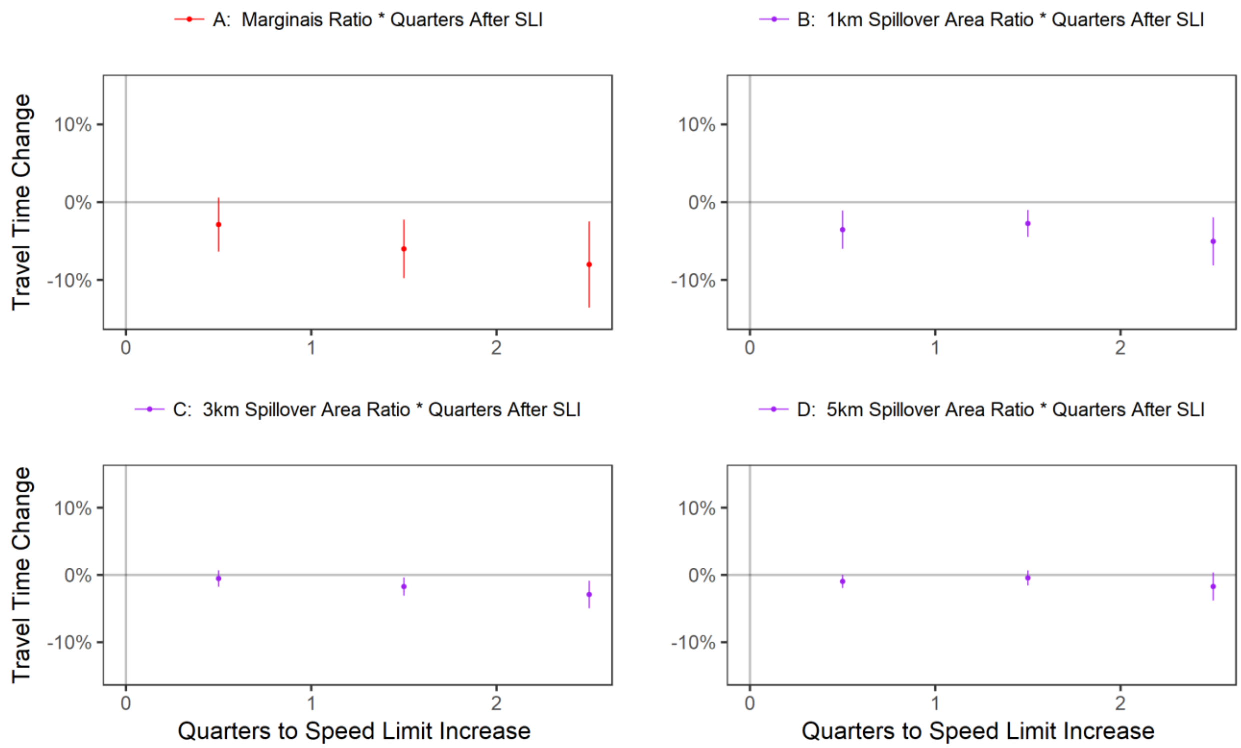

Figure 1.13: Event-Study Results: Changes in Travel Time After the Speed Limit Increase on the Marginais

1.4 Cost-Benefit Analysis

To compare the social costs and benefits of the speed limit changes that were implemented in São Paulo, we first assign a monetary value to road accidents and travel time using standard parameters from the literature. Next, given the policy impacts estimated in our previous session, we construct counterfactual scenarios where we calculate accident costs and travel time losses under alternative speed limit policies. We compare the social costs and benefits calculated in these counterfactual scenarios with the values that were actually observed, thus computing the total costs and benefits for each of the policies changes that were implemented in the city. Next, we focus our analysis in the case of the speed limit reduction in the *Marginais,* where we are able to net out the effect of enforcement, and compare the specific effect of speed limit reduction on accidents and travel time. Moreover, by exploring the individual level characteristics of our datasets, we evaluate the distribution of these specific costs and benefits throughout different groups of society.

1.4.1 Social Benefits of the 2015 Speed Limit Reductions

Table 1.13 summarizes our results for the benefits associated with the speed limit reductions of 2015. The first row of the table indicates the total monetary costs of road accidents observed on business days[23] of 2016 where each column corresponds to different road group. The total cost of accidents is estimated in R$ 2.8 billion, and the share of this amount associated with accidents that take place on the *Marginais*, arterial, and other roads is of respectively 3%, 35%, and 62%. Next, based on the policy impacts estimated in session 3.1 we calculate a series of counterfactual scenarios where we simulate what would be the costs of accidents in São Paulo if the speed limit reduction of 2015 had not been implemented.

Table 1.13: Yearly Benefits due to Accidents Reduction After the Speed Limit Reductions of 2015

Scenarios[a]	Accident Costs (BRL Million)			
	Total	Marginais	Arterial Roads	Other Roads
Observed				
Observed Costs	2,805.9	86.4	985.3	1,734.2
Share	1.00	0.03	0.35	0.62
Speed Limit, Cameras and Spillovers				
Counterfactual Costs	3,546.9	175.3	1,389.7	1,981.9
Policy Benefits	741.0	88.9	404.4	247.7
Share of Benefits	1.00	0.12	0.55	0.33
Speed Limit and Cameras				
Counterfactual Costs	3,299.2	175.3	1,389.7	1,734.2
Policy Benefits	493.3	88.9	404.4	-
Share of Benefits	1.00	0.18	0.82	-
Speed Limit Effects Only				
Counterfactual Costs	3,250.1	156.8	1,359.0	1,734.2
Policy Benefits	444.2	70.4	373.7	-
Share of Benefits	1.00	0.16	0.84	-

Notes: [a] All scenarios are based on the road accidents observed in 2016 businessdays. Counterfactual scenarios are based on the policy impacts estimated from our reduced form empirical models. All values are converted to BRL of 2016 based on IBGE IPCA inflation index

[23] We limit our analysis to business days because our underlying household travel survey is only representative to those days.

First, we estimate the total increase in road accidents if we consider all the effects for speed limit reduction, interaction with camera enforcement, and spillovers effects on non-treated roads. In this case, the total policy benefit would be of R$ 741 million, with 33% of this amount deriving from spillover effects. Next, we estimated a more conservative counterfactual where we assume null spillover effects on non-treated roads. In this case, the total policy benefit would be of about R$ 493 million per year. Finally, in our last simulation, we exclude the enforcement effect and its interaction with the speed limit change, hence isolating the policy effect due to speed limit reductions only. In this case, we estimate a total policy benefit of R$ 444 million, out of which R$ 70.4 would be due to accident reduction in the Marginais.

1.4.2 Social Benefits of the 2017 Speed Limit Increase

Now, moving to the other side of the cost benefit analysis, we calculate the monetary value of time savings using two alternative parameters for the value of time (VOT). First, as our central case, and similar to van Benthem (2015), we use the after-taxes hourly wage of individuals. Although we use this parameter as our central case so we can compare our results with Van Benthem, this value should be seen as an upper bound for the policy welfare impacts on travel time. The USDOT recommends assigning half of the hourly wage for non-business trips within local urban settings (USDOT, 2014), meanwhile, the empirical literature has consistently identified an average VOT of approximately 50% of wage rate (Wolff, 2014). Therefore, as an alternative, and possibly more reasonable parameter, we also calculate policy benefits using the Victoria Transport Policy Institute (VTPI) guidelines which suggests assigning a VOT of 150% of travelers' wage for business trips, 50% for commuting (35% if passenger), 25% for personal travel, and 0% for leisure or vacation (Victoria Transport Policy Institute, 2016).

Table 1.14 reports the results of our calculations based on these two different parameters. The first set of columns refer to the results using a the after-tax hourly wage VOT, and the second set reports the results using the VTPI VOT. Like in the case of accidents, the first line of the table shows the baseline total cost of time in traffic observed in 2016, that is, accounting for the fact that the speed limit in the *Marginais* was 70km/h in that year. Next, we calculate two distinct

Table 1.14: Yearly Benefits due to Travel Time Savings After the 2017 Reduction of Travel Time

Scenarios[a]	Cost of Time Spent in Traffic (BRL Million)							
	VOT = After-Tax Wages				VOT = VTPI guidelines			
	Total	Marginais	Spillover Area	Other Roads	Total	Marginais	Spillover Area	Other Roads
Observed								
Observed Costs	15,361.6	1,170.6	3,261.3	10,929.7	6,286.1	524.5	1,327.0	4,434.5
SLI Marginais + Spillovers								
Counterfactual Costs	15,226.8	1,113.8	3,183.3	10,929.7	6,228.9	499.1	1,295.3	4,434.5
Policy Benefits	134.8	56.8	78.0	-	57.2	25.5	31.7	-
SLI Effects - Marginais Only								
Counterfactual Costs	15,304.8	1,113.8	3,261.3	10,929.7	6,260.6	499.1	1,327.0	4,434.5
Policy Benefits	56.8	56.8	-	-	25.5	25.5	-	-

Notes: All scenarios are based on the expansion of household survey trips made by private vehicle to all business days of 2016. Counterfactual scenarios are based on our empirical results of policy impacts. All values are converted to BRL of 2016 by the IBGE IPCA inflation index.

counterfactuals for the speed limit increase to 90km/h; first including spillover effects on nearby roads, and second restricting the travel time effects to treated roads only. In the first case, the value of total travel time savings would be of R$ 134.8 million if we use our upper bound VOT and R$ 57.4 million if adopt the VTPI VOT. However, if we exclude the spillover effects, those values decrease to respectively R$ 56.8 million and R$ 25.5 million.

1.4.3 Comparing the Cost and Benefits of Speed Limit Changes in the Marginais

It is important to notice that the speed limit reductions of 2015 and the speed limit reversal of 2017 should not be directly compared in their totality because the former affected the city highways and arterial roads, and included a large expansion of camera enforcement. Meanwhile, the speed limit reversal of 2017 was restricted to the *Marginais* and was not accompanied by any substantial changes in enforcement. Therefore, to narrow down the comparison of policy costs and benefits, we focus on the speed limit changes in the *Marginais*. For these particular roads, we have from our reduced form results: 1) an estimation of benefits due to accident reductions (R$ 70.4 million) from the 2015 policy that excludes the effects of camera enforcement; 2) a range of estimations of travel time savings (R$ 25.5 – R$ 56.8 million) due to the 2017 speed limit reversal.[24] Hence, if we assume that travel time effects observed in 2017 were the inverse of the 2015 speed limit reduction impacts, we are able to isolate and compare the benefits and costs associated with the 2015 speed limit reduction in the *Marginais.* Hence, if we accept this assumption of an inverted travel time effect given a speed limit increase or reduction, we can calculate that the benefit/cost ratio of the 2015 speed limit reduction was of approximately 1.23 if we use our upper bound VOT, and to 2.76 if we consider the VTPI VOT. As a conservative approach, we chose the first of these values as our central case for the following analysis.

Next, we compare the sensitivity of this result given alternative parameters for the Value of Statistical Life (VSL), which is the main driver of policy costs associated with road accidents. Given the underlying uncertainty about this parameter, an interesting value to be identified is the minimum VSL for which a speed limit reduction would still be socially worthwhile, which we define as the policy breakeven VSL. Figure 1.14 shows the result of this analysis. The x-axis indicates a continuous of VSL values, and we highlight with vertical lines the following values: 1) the breakeven VSL for our central case of total policy costs where VOT is calculated at after-tax hourly wages; 2) our baseline VSL parameter of R$ 4.54 million taken from Viscusi & Masterman (2017); 3) the EPA recommended VSL of R$ 20.1 million for the USA. Moreover, the horizontal lines indicate the total cost of the speed limit reduction given our alternative VOTs parameters.

The results of this analysis indicate that the speed limit reductions in the *Marginais* would still be socially worthwhile up to a VSL larger than R$ 3.2 million, or about 70% of our central case value. However, if we consider the VTPI VOT parameters, the breakeven VSL would approach zero as the reduction of costs from non-fatal accidents would already be sufficiently large to compensate for the increases in commuting time. Finally, if we consider the VSL suggested by the USA EPA, the policy benefits would be more than 3.6 times larger than our maximum estimated policy costs.

[24] In our cost-benefit analysis we do not include the spillover effects from travel time and accidents because in the case of accidents, we cannot isolate the spillover effects from the treatment of the *Marginais* from the treatment of arterial roads and their corresponding spillovers. However, given the underlying mechanisms that associate travel speed and the probability of road accidents, our evidence indicates that any spillover effects on travel time would be accompanied by a corresponding effect on accidents. Therefore, by excluding the spillover effects from both travel time savings and accidents, we should not change dramatically the cost benefit-ratio of police if compared to an evaluation that was able to incorporate both effects. If the dataset of accidents from 2017 is released, we will be able to empirically test the effects of the 2017 speed limit increase on accidents and its corresponding spillovers to nearby roads.

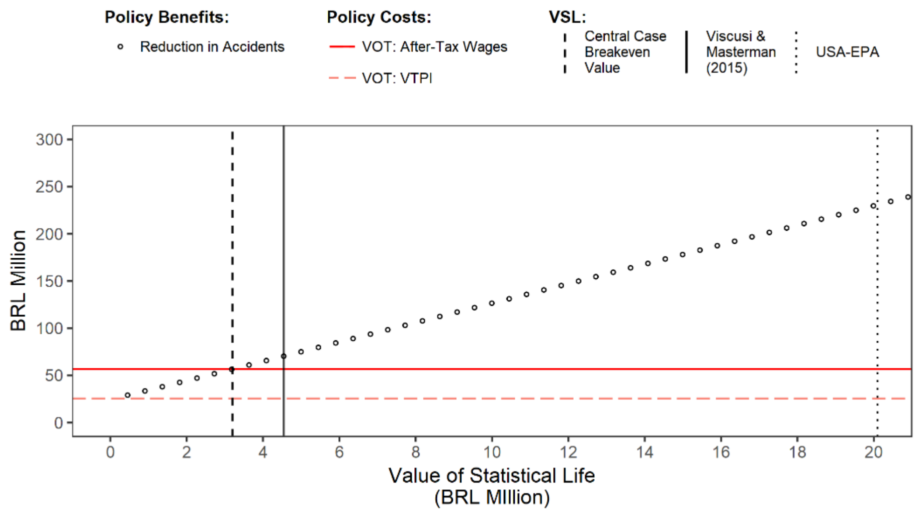

Figure 1.14: Cost-Benefit Analysis: Sensitivity of Results to Alternative Parameters

1.4.4 Distributive Analysis of Speed Limit Changes in the *Marginais*

Next, we take our central case parameters and evaluate the distribution of policy costs and benefits throughout different segments of society. Common among our databases, we have the education attainment of commuters from our household travel survey, and accident victims from our database of road accidents. Although we do not have income information about accident victims, we base our distributive considerations on the strong correlation between education attainment and wealth. Figure 1.15 evidences this relationship for the case of São Paulo by plotting the average income of adults (age > 18) from the household survey by education attainment.

To compute our distributive analysis, we assume that all the effects of the speed limit reduction on road accidents and travel time are equally distributed irrespectively of individuals education attainment. For example, if two individuals with distinct income levels are observed making the exact same trips on the *Marginais*, then we assume that their corresponding travel time changes after the speed limit reduction should be the same. Moreover, since on the benefit side we only have an average VSL for the whole population, we also compute the distribution of costs using a single average VOT to avoid different skewing of the cost-benefit components.[25]

[25] Although it would be possible to calculate individual specific VSLs with our data, that would imply assigning distinct values to the life of individuals from different income strata. We avoid the ethical concerns about such calculation by using population averages only.

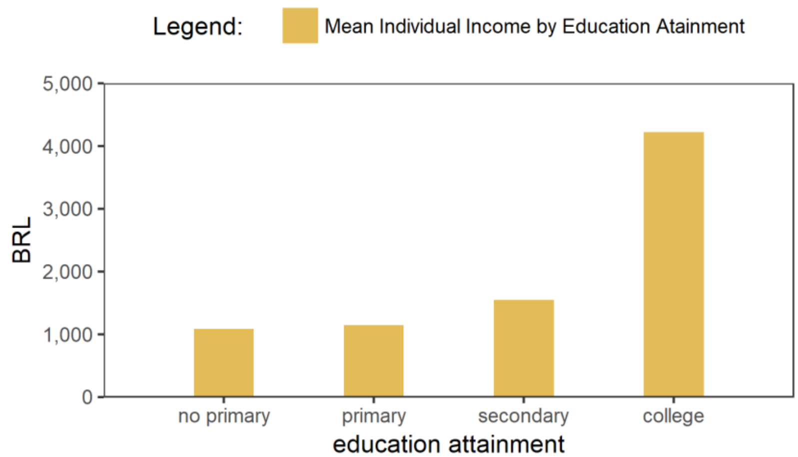

Figure 1.15: Average Income by Education Attainment in São Paulo, Brazil

Given these assumption, Figure 1.16 plots the mean costs and benefits of the speed limit reduction in the *Marginais* by education attainment of individuals in São Paulo. Since groups differ in terms of total number of individuals, we present the results in per capita terms. The figure indicates a clear distinction in the distribution of costs and benefits. While the benefits are larger for individuals with no primary, primary and secondary education the travel time costs seem to have a particularly larger effect on the group of individuals with college education, which becomes the only group for which travel time losses are larger than accident reduction benefits.

To investigate the mechanisms behind these distributive effects, we investigate two variables: 1) since the main component of accident costs are fatalities, we verify the distribution of road deaths by education; 2) on the cost side, we examine the use of private vehicles by group. The resulting diagrams of these variables are presented on Figure 1.17, which shows on the y-axis the relative participation of each of these variables compared to the overall population of São Paulo. A negative value indicates that the participation of individuals from the corresponding group is below the share of individuals from this group in the population of São Paulo. A positive value means the opposite.

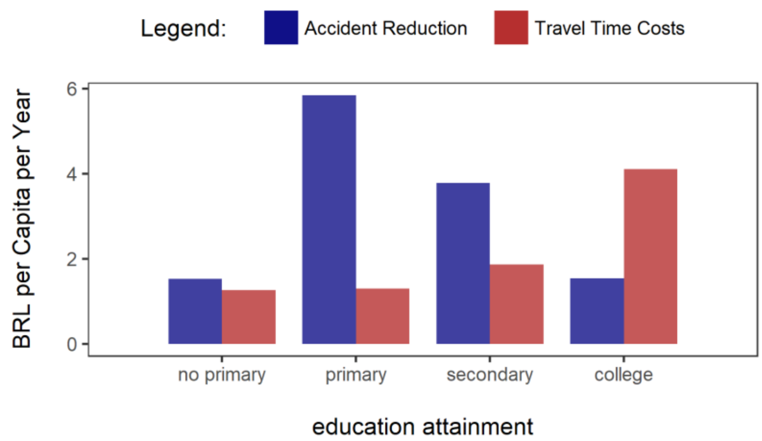

Figure 1.16: Per Capita Distribution of Cost and Benefits by Education Attainment: Speed Limit Reduction at the Marginais

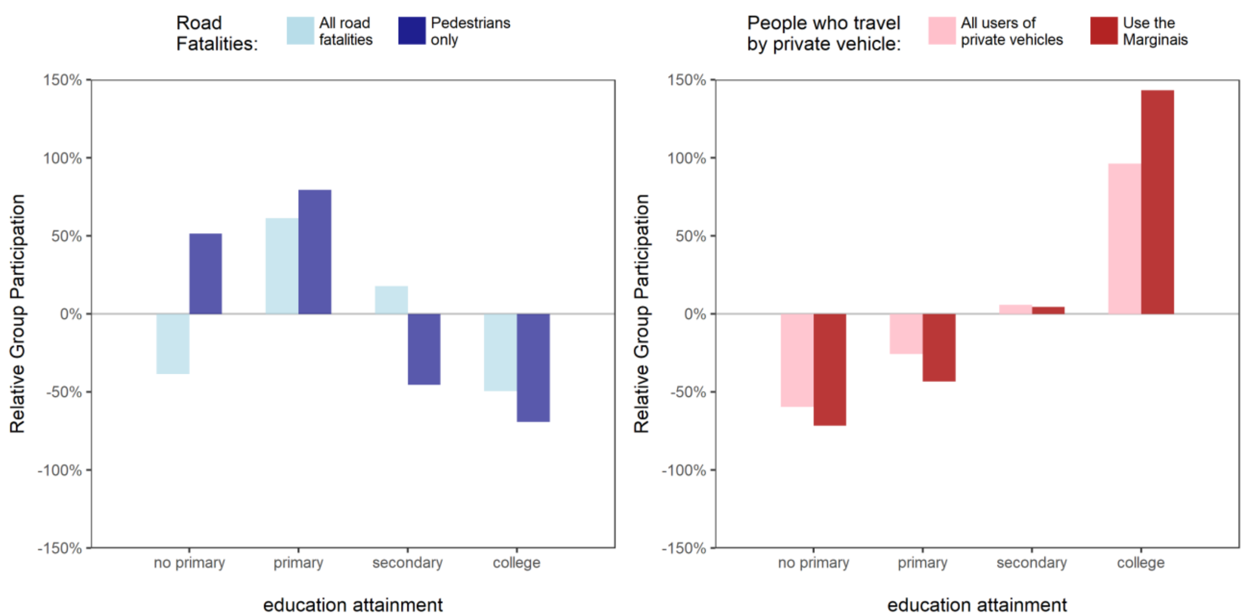

Figure 1.17: Average Income by Education Attainment in São Paulo, Brazil

The use of private vehicles is relatively larger for individuals of higher income levels. Moreover, in the specific case of people who use private vehicles at the *Marginais*, the concentration is even more accentuated. Meanwhile, the relative distribution of road fatalities is larger for the individuals with primary education, and in the case of pedestrians, it is also larger for those below primary attainment.

Therefore, the heterogeneity of impacts from the speed limit reduction on the *Marginais* can be explained by these two factors. On one hand, people who use private vehicles and are more heavily harmed by longer commuting time tend to be wealthier. On the other hand, road fatalities are not evenly distributed in the society, as lower and lower-middle income individuals are overrepresented as the fatal victims of road accidents.[26]

1.5 Comparison of Results with the Literature

To the extent of our knowledge, within the applied economic literature, the study that most closely approximates to our paper is van Benthem (2015), henceforth VB. Although both paper conduct an ex-post evaluation of impacts from speed limit changes, it is important to notice that there are important differences with respect to the setting evaluated in the papers. While VB examines speed limit changes from more than 20 years ago on regional freeways in western USA[27], our study examines a policy change from 2015 on urban roads in a metropolitan area of the developing world. Therefore, when comparing the results from both papers, it is important to acknowledge that each of these distinct dimensions may play an important role in explaining the differences between their results.

Importantly, VB evaluated the effects of a speed limit increase, and us, a speed limit reduction. Therefore, to align the direction of results, we invert his labels of costs and benefits, so we use "costs" to refer to the value of additional travel time, and "benefits" to refer to reductions in accidents.

[26] Possible reasons for these differences in fatality rates may include the proportion of travelers who use motorcycle, the ability to afford safety features on vehicles, and differences in driving behavior. Although this is a highly relevant question, identifying the precise mechanisms that drive the road mortality differentials is beyond the scope of our paper.

[27] The roads evaluated in van Benthem (2015) were located in the states of California, Oregon and Washington.

Table 1.15: Comparison of Results with van Benthem (2015)

	Ang, Christensen & Vieira (2018) *Urban Highways in São Paulo, Brazil*	Van Benthem (2015) *Western USA Freeways*	Ratio *ACV/VB*
Cost-Benefit Results			
Benefits/Costs	1.46	2.21	0.66
Breakeven VSL ratio[a]	0.70	0.50	1.39
Main Parameters			
VSL *(U$ Million)*	1.72	8.78	0.20
VOT *(U$ per hour)*	3.89	18.31	0.21
Reduced Form Estimates			
Travel Time	0.057	0.059	0.97
Accidents			
non-fatal	0.47	0.14	3.37
fatal	0.21	0.44	0.49

Notes: All monetary values are in USD of 2016. [a] Breakeven VSL divided by baseline parameter

Table 1.15 summarizes the comparable results between the two studies.[28] First, it is worth noticing that the VSL and VOT parameters used in our study were both about 20% of the main parameters used by VB, which facilitates the comparison of results as we have no major differences in the VSL/VOT ratio.

With respect to reduced form policy impacts we have found very similar effects of the speed limit changes on travel time, with an average effect of approximately 6% in the same direction as the limit change. However, the main difference between the studies was the estimation of impacts on accidents. While we found a decrease of non-fatal accidents that was 3.37 larger in magnitude, our estimated policy impact on road fatalities was about half of his. The difference in non-fatal accidents is an important result as it is precisely estimated in our model. If we calculate policy costs using the VTPI VOT, the breakeven VSL approaches zero as the changes in non-fatal accidents alone compensate the losses in travel time.

[28] Besides the items listed here, van Benthem (2015) results include the effects of the speed limit changes on air pollution and its consequent impacts on health. While these effects are estimated to approximately double the social costs of speed limit increases, they are which are admittedly uncertain by the author. Unfortunately, we lack the required data to carry a similar analysis on this effect. Therefore, when comparing the results between the papers, we exclude Van Benthem results associated with pollution and health impacts.

In both studies, the benefits related to fewer accidents were found to be larger than the costs due to longer travel times. However, while in his study the central benefit/cost ratio was of 2.21, in our case the same ratio was about 1.46. Moreover, the relative breakeven VSL was about 40% higher in our study, which is also an indicator of smaller policy benefits in our setting. There are different factors that contribute to this differences in ratios, but primarily, our lower effect on fatal accidents is key as road fatalities are the main components of road accident costs.

In synthesis, compared to VB, our study has also identified that the gains associated with lower accidents are larger than travel time losses. This result was found while using a VOT that is admittedly likely to overestimate the costs of travel time if compared to recommended parameters from the literature. Given that we estimated the impacts of a speed limit reduction in a completely different setting from VB, the similarity of results supports the external validity for claiming that speed limit policies are an effective tool for reducing road accidents without imposing net welfare losses to society.

1.6 Conclusion

This paper has analyzed the outcomes associated with a set of policies that altered traffic speed limits in São Paulo Brazil. We have identified that after a series of speed limit reductions in 2015, there was a large decrease in road accidents and fatalities on treated road segments. We've shown that this reduction in accidents cannot be attributed to road substitution or pre-intervention downward trends. Moreover, by scrapping data about traffic congestion from a web API, we were able to identify that after a speed limit increase adopted in 2017, the estimated travel time of trips using the affected roads has gone down. We also shown no evidence of road substitution associated with this policy.

Combining these results with standard parameters from the literature, we compared the costs and benefits of the speed limit reduction adopted in the city urban highways in 2015. According to our calculations, the benefits of the policy outweighed its costs even in our most conservative choice of parameters. Moreover, we've shown that the policy had a progressive impact as the reduction in accidents had a larger impact for lower- and middle-income groups, while travel time losses were larger for wealthier individuals.

We acknowledge that our analysis is not final for a complete assessment of speed limit changes impacts. Our representative sample of trips constrains our analysis to trips made by residents on business days, so we do not account for policy impacts on freight transportation and trips made by non-residents. Therefore, we may not be accounting for an important share of social costs associated with a speed limit reduction. On the other hand, we also do not account for the policy benefits associated with pollution and health, which although uncertain, are estimated by Van Benthem (2015) to double the assessment of policy benefits.

Moreover, an important aspect which is specific about our setting and we have not fully explored is the magnitude of the enforcement structure employed in São Paulo. In the year of 2016, the city issued 6.7 million speeding tickets, more than half a ticket per resident. The local government of São Paulo collected approximately R$ 1.1 billion from these tickets, an amount that is larger than our overall assessment of gross policy benefits associated with all speed limit reductions adopted in 2015. In principle, this amount corresponds to a mere transference of resources, so it should not affect the social welfare impacts of the policy. However, given its magnitude, it imposes non-negligible distributive effects that we have not considered in our analysis and certainly deserve a further investigation.

As for the external validity of our results, it is obviously limited given the case-study nature of our analysis. However, several cities throughout the world are recently experimenting with stricter speed limit policies, therefore an interesting exercise would be to compare outcomes similar to the ones obtained in our study to evaluate the heterogeneity of policy impacts on different types of urban areas.

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Chapter 2: Affirmative Action in Brazilian Universities: Effects on the Enrollment of Targeted Groups

With Mary Arends-Kuenning

This paper investigates how the adoption of affirmative action for college admission affected the enrollment of students from socioeconomically disadvantaged groups in Brazil. We explore the time heterogeneity of policy adoption by Brazilian universities to identify the policy effects while accounting for time confounding unobservables. Our analysis indicate that the adoption of affirmative action policies led to an increase in the enrollment of students from groups explicitly targeted by each policy, particularly public high-school students and Blacks.[29] *As for the heterogeneities of policy impacts, larger effects were observed for more competitive and more prestigious academic programs. Lastly, we found that universities that adopted affirmative action policies with explicit racial criteria experienced an increase in the enrollment of Black students; meanwhile, universities that adopted race-blind policies had no significant changes in the racial profile of their admitted students.*

Keywords: Affirmative Action, Access to Higher Education, Race-Blind vs. Race-Conscious Policies

Affirmative action policy (AAP) for college admission is a common practice worldwide; it has the objective of mitigating discrimination by providing access to educational opportunities that otherwise would not be available to individuals from disadvantaged groups. This study examines the adoption of such policies in Brazil, one of the most unequal countries in the world[30] and where

[29] The standard racial/skin-color categories used by the Brazilian Statistical Agency (IBGE) include: *Branco* (light-skinned), *Preto* (black-skinned), *Amarelo* (yellow – mainly referring to Chinese and Japanese origin), *Pardo* (brown-skinned and/or mixed) and *Indígena* (Native American or indigenous). The Portuguese term "*Pardo"* is especially ambiguous (Cicalò, 2008), and any direct translation to English may be misleading. Therefore, following other English-written studies on the topic, we use the original Portuguese terms to refer to the standard racial categories used in Brazil. However, Brazilian affirmative action policies were often defined to target the combined categories of *Pretos and Pardos*, which are denoted in Portuguese as "*Negros"*. To avoid any possible confusion with the English term "Negro", which may have a derogatory connotation, we use the English word "*Blacks"* to refer to the combined groups of *Pretos* and *Pardos*.

[30] With a Gini coefficient of 52.87, Brazil ranks 10th among world countries in terms of inequality as measured by this coefficient (World Bank, 2017).

the educational opportunity gap between individuals of different socioeconomic strata is one of the main channels of intergenerational inequality persistence (Barros, Foguel, & Ulyssea, 2006). Barriers to access to tertiary education are viewed as particularly relevant for reducing inequality because returns to college degrees are exceptionally high in Brazil (OECD, 2016).[31] Not surprisingly, limited access to higher education is an important mechanism restricting income mobility in the country (Ferreira & Veloso, 2006). Therefore, with the objective of improving the access of deprived individuals to higher education, Brazilian universities started experimenting with AAPs in the early 2000s, and within less than a decade, most public colleges had adopted some type of AAP for selecting their students. However, it is still not clear how effective these policies were in changing the profile of students enrolled in these academic programs.

This paper aims to answer that question by investigating the impacts of AAP adoption on the profile of students enrolled in academic program subject to those policies. To identify the causal effects of AAPs, we explore the heterogeneity of policy adoption by different universities between 2004 and 2012, a period when each Brazilian federal institution had full discretion to define their own set of admission policies. We explore a rich dataset containing socioeconomic information from a large sample of freshmen students from all Brazilian federal universities. Using a difference-in-differences estimation strategy, we compare changes in the demographic and socioeconomic characteristics of students enrolled in Universities that introduced AAP with changes observed in institutions that did not adopt those policies. By including a control group, our estimates account for unobserved shocks that were concurrent to the adoption of AAP and that may have affected the selection of students in Brazilian higher education irrespectively of AAP adoption. Moreover, because our dataset includes a large number of academic programs from all Brazilian federal universities, we are able to further investigate the heterogeneity of policy effects. First, we examine how impacts differed with respect to program competitiveness. Second, we evaluate the outcomes of distinct types of policies, contrasting race-blind with race-conscious AAPs.

Our results indicate that the AAPs evaluated in our study were effective in increasing the enrollment of individuals from groups explicitly targeted by each policy, particularly students who

[31] The OECD report indicates that, in Brazil, someone with a college degree earns on average 3.4 times more than someone with completed secondary education only. This differential is the highest among all OECD and partner countries (OECD, 2016).

graduated from public high-schools, a characteristic strongly associated with socioeconomic status and which was the most common eligibility criteria for the policies adopted in the period of our analysis. Moreover, while the impacts were larger for more prestigious academic programs, they were mostly negligible for less competitive ones. Finally, and most importantly, we observed that the outcomes of the policies were limited in the case of deprived groups not explicitly targeted by each policy. For example, the enrollment of Black students was mostly unaffected by APPs with race-blind eligibility criteria. In contrast, race-conscious policies led to a significant increase in the enrollment of Blacks.

Compared to the existing empirical literature, our study has the novelty of evaluating the outcomes of AAP in a setting where universities employ objective and publicly known criteria for student selection, but where institutions had discretion to experiment with different types of admission policies. In American universities — which are the object of study of the largest portion of the literature — the selection of students is based on a complex and subjective set of attributes, and universities are not required to disclose the weight of racial preferences in their admission processes, hence the exact effects of AAPs on student selection are not identifiable. Meanwhile in India, — which is another country with an extensive literature investigating the effects of AAP — public universities are required to comply with specific AAPs that are imposed by the government. Therefore, in the Indian case, there is limited heterogeneity in the policies adopted by each university, precluding the empirical comparison of outcomes from different types of policies.

Our study also differs from the existing literature that examines the effects of AAPs in Brazil which are mostly restricted to the outcomes of individual university experiences. In contrast, our analyses comprise all Brazilian federal universities, including the ones that had not adopted any type of AAP in the period of our evaluation. That is an important contribution of our study because concomitantly with the adoption of AAPs by Brazilian universities, structural changes were taking place in the country with far-reaching impacts on the application and selection of students to tertiary education. First, the total number of undergraduate positions increased from 3.03 million in 2001 to 5.45 million in 2010. Most of this growth was associated with a large expansion of private universities, which were boosted by a set of federal policies aimed to help lower income students to cover and finance their college tuition expenses.[32] Moreover, during this period, Brazil was experiencing a positive economic cycle, with extensive impacts on income distribution and employment.[33] Each of these factors had important impacts on the pool of students applying to and enrolling in Brazilian public universities. Hence, the isolated effects of AAP on the enrollment of deprived students is hindered by the simultaneity of these processes, and the evaluation of policy outcomes that are based on a single university's experiences are likely to be overestimated as they cannot account for the effects of these simultaneous processes.

In synthesis, our paper has the novelty of examining the effects of AAPs on the enrollment of students from targeted deprived groups based on an ex-post evaluation of policy impacts on a large number of universities and several types of academic programs, accounting for time-confounding structural changes and comparing the effects of different types of AAPs in a setting where the criteria for student selection were transparent and different types of policies were adopted by different institutions.

The remainder of the paper is divided as follows. Section I describes in further details the use of AAPs for student admission and revisits the existing empirical literature about its impacts and the institutional background of its adoption in Brazil. Section II details our data and Section III presents our empirical strategy and discusses its results. Finally, Section IV concludes.

2.1 Background

2.1.1 AAPs for College Admission throughout the world

In the international context, selection processes with specific rules to favor individuals from historically disadvantaged groups date from as early as the first years of the Twentieth

[32] The most important of such policies were the *Student Financing Fund for Higher Education* (FIES) and the *University for All Program* (PROUNI). The first of these policies, FIES, was established in 1999, and it was designed to provide subsidized credit for students to finance their tuition and fees in private universities. By 2004, the program included 312,000 contracts, corresponding to about 10% of all students enrolled on private universities. In that same year, the Federal government created an additional program, PROUNI, a program that offered partial and total scholarships for students enrolled on private institutions. By 2013, 37.3% of private university students were beneficiaries of FIES and 11.9% of PROUNI (Corbucci, Kubota, & Meira, 2016).

[33] Between 2003 and 2014, the real income level of the Brazilian poorest 40% rose 7.1% per year (World Bank, 2017).

Century when the first reservation policies were adopted in colonial British India (Laskar, 2010). However, the term "affirmative action" was first coined in the 1960s when the U.S. presidency passed a set of executive orders with the objective of addressing the country's historical legacy of discrimination against minorities, particularly African Americans. Subsequently, several American universities voluntarily implemented AAPs giving preferential treatment to candidates from minority groups (Holzer & Neumark, 2006). Following this initial process, a contentious debate emerged about the use of AAP for college admission in the U.S., ultimately resulting in a set of Supreme Court landmark decisions on the legality of these policies. Still, AAP remains one of the most controversial policy topics not only in the U.S. but in other countries where it is practiced.

Interconnected with the public debate, the academic literature investigating the effectiveness and other aspects of AAPs is also extensive,[34] although the majority of empirical studies are still limited to the evaluation of policy outcomes in the USA. Yet, in recent years, the adoption of AAPs in other countries has gained broader attention from researchers, particularly the experiences of Indian universities. Meanwhile, other major countries have also experimented with AAPs for college admission, including China, Brazil, South Africa, and Malaysia among others. However, the empirical investigation of policy impacts is still limited in the case of these nations.

In the USA, the existing literature indicates that AAPs have increased the probability of racial minorities to be admitted to and enrolled in American universities, particularly in top-tier colleges (Epple, Romano, & Sieg, 2008), (Long, 2004), (Arcidiacono, 2005). However, the precise effect of these policies is not easily identifiable because American universities are not required to disclose the weight of racial preference in their admission processes (Holzer & Neumark, 2006). In recent years, state bans on race-based admission policies have been explored by empirical researchers to estimate the impacts of AAP on the admission and enrollment of minorities. For instance, Hinrichs (2012) and Backes (2012) report substantial reductions of African Americans, Hispanics and Native American enrollments in top American colleges after these bans. Once again, effects on non-top colleges were found to be mostly negligible.

[34] For a general review of the literature on AAP, see (Holzer & Neumark, 2006).

Meanwhile, Card & Krueger (2005) showed that applications to colleges by minority students with high academic performance were not affected by the policy bans, so the enrollment reductions of minorities could not be explained by a diversion in the applications of AAP targeted students.

An important question of interest about the outcomes of AAPs in the USA relates to the effectiveness of Race-Blind AAPs on indirectly improving access to college for racial minorities. Favoring individuals based on race is one of the most controversial aspects of AAPs, therefore race-blind policies are less likely to be politically and/or legally rejected. Examples of race-blind AAPs are the "top-x%" programs, which grant admission to students ranked in the top x percentile of their high-school cohort. Because of the legal and political challenges to race-based AAPs in some American states, these programs were designed to increase university access for minority students without explicitly targeting race or ethnicity, basically by exploiting the racial segregation of American neighborhoods. Long (2004) argued that these programs would not be able to achieve the same outcomes as explicitly race-conscious AAPs. However, Kapor (2016) showed that the Texas top 10% program increased the enrollment of minority students by about 10% at Texas flagship universities and attracted students with stronger academic performances compared to typical race-conscious AAPs. This result suggests that well designed race-blind policies could achieve the outcome of significantly improving access to opportunities for underprivileged racial minorities. However, the generalization of such a result to other experiences is still an empirical question.

With respect to the outcomes of AAP in the developing world, the experiences of Indian universities have attracted the attention of applied economists in recent years. In contrast to the American setting, Indian universities normally rely on objective and straightforward methods for admission. Students applying to college are ranked based on their performance on standardized exams, and the top ranked students are selected for admission. Therefore, the effects of AAPs in changing the enrollment of students from deprived groups can be directly computed (Frisancho & Krishna, 2016). Moreover, detailed databases of students are available, allowing researchers to follow not only the applicants who were admitted to a certain college, but also those who were rejected. As a result, in the case of Indian universities it is possible to identify the exact students who were admitted and displaced due to AAPs, and further investigate their academic performance and post-educational outcomes. By tracking individuals who applied to Indian engineering colleges with quotas for certain castes, Bertrand, Hanna, & Mullainathan (2010) showed that those who were admitted directly because of AAP came from less wealthy households than the displaced applicants. Moreover, policy beneficiaries experienced positive returns to labor market earnings due to their college enrollment. Similarly, Bagde, Epple, & Taylor (2016) reported that the Indian AAPs in engineering colleges increased enrolment of students from targeted groups, with larger effects for the most disadvantaged castes and no evidence of college "mismatch" associated with the policy.

However, the evaluations of AAP outcomes from Indian experiences are still limited in at least two dimensions. First, the existing empirical studies are restricted to a subset of academic programs, most commonly engineering, so the heterogeneity of policy effects in different careers has still not been investigated. More importantly, the AAPs practiced by Indian colleges follow government regulations that require that a certain share of positions must be reserved for disadvantaged castes in all public institutions within a same state (Bertrand, Hanna, & Mullainathan, 2010). Therefore, in the Indian setting, the AAPs adopted by different public universities are mostly homogeneous, limiting the empirical investigation of the differences in outcomes of distinct types of policies.

2.1.2 AAPs for college admission in Brazil

Brazil was the destination of approximately half of all enslaved individuals brought to the Americas during the Atlantic Slave Trade. It was the last Western country to abolish slavery, and consequently, it is still one of the most unequal societies in the world.[35] One of the main mechanisms through which the institution of slavery leads to long-term impacts on income inequality is through human capital accumulation (Bertocchi & Dimico, 2014). Despite this historical background of slavery and an extreme level of socioeconomic inequality, Brazil only started adopting AAPs in the early 2000s, about 40 years after the USA and a century later than the first Indian reservation policies.

[35] A recent economic literature has been devoted to identify the long term impacts of slavery on inequality, including (Bertocchi & Dimico, 2014), (Soares, Assunção, & Goulart, 2012), (Fujiwara, Laudares, & Caicedo, 2017).

The pioneering experience with AAP for college admission in Brazil was the Program of Quotas at the State University of Rio de Janeiro (UERJ), which was introduced in 2003 and reserved 45% of the university positions for PHSS, Blacks, *Indígenas*, and students with physical disabilities. In 2004, the National University of Brasília (UNB) became the first federal university[36] to introduce an AAP for selecting its students. It established a system of quotas reserving 20% of its positions for Black applicants. In the following years, several other public institutions created their own set of AAPs, and by the end of that decade, most federal universities had adopted some type of AAP in their admission processes.

In 2012, the Brazilian federal government passed a new law which led to an unprecedented expansion in the use of AAP for college admission in the country.[37] Like in the Indian case, the Law of Quotas limited the heterogeneity of AAPs in Brazilian federal universities as all institutions were obligated to follow the AAP rules imposed by the Law. However, the analyses presented in this paper cover the period that goes from the first experiences with AAP in the early 2000s to before the Approval of the Law of Quotas in 2012. During this period, Brazilian federal universities were allowed to define their own set of AAPs, including the alternative of not adopting any AAP at all.

Because the Brazilian experience with AAPs for college admission is relatively recent, the empirical literature investigating the impacts of these policies is still narrower than in the case of the USA or India. With respect to the policy effects on the enrollment of targeted students, most studies are restricted to the outcomes of specific university experiences. Examples of such studies include Cicalò (2008), Francis and Tannuri-Pianto (2011), Aranha, Pena, & Ribeiro (2012), and Estevan, Gall, & Morin (2016), each investigating the outcomes of AAPs adopted in different Brazilian public universities.

Cicalò (2008) studied the case of UERJ, observing that the number of policy beneficiaries enrolled in the university had consistently decreased after the adoption of AAP for students'

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[36] Brazilian Higher Education Institutions can be divided into two main categories, public and private. Public Universities are most commonly tuition free, and in 2004 they accounted for 28% of Brazilian students in tertiary education. Public Universities can be further separated into three groups: federal, state and municipal institutions, respectively accounting for 48.8%, 40.0% and 11.2% of student in public institutions (INEP, 2005).

[37] The national Law of Quotas specified that all federal universities should reserve, by 2016, half of its undergraduate positions to applicants from disadvantaged groups, including PHSS, Blacks, Indígenas and lower income individuals. The law required universities to start reserving its positions in 2013, with a gradually increasing quota share up to 2016.

admission, particularly in less competitive careers. The author argues that the policy may have saturated the demand of targeted groups for less prestigious programs.

Francis and Tannuri-Pianto (2011) examined the case of UNB, which reserved 20% of undergraduate positions to candidates who self-declared as Blacks.[38] Their analysis indicates that the share *Pretos* and *Pardos* increased after the policy adoption. Additionally, they identify that students selected through the system of quotas were more likely to come from lower socioeconomic backgrounds than displaced applicants. Importantly, their study revealed that dark-skinned candidates were more likely to identify themselves as *Pretos* and *Pardos* when compared to the period before the implementation of AAP, indicating that at least part of the policy effect could be attributed to a shift in the racial self-classification of students.

Aranha, Pena, & Ribeiro (2012) studied the policy of *Bonus* adopted by the *Federal University of Minas Gerais* (UFMG) in 2009. The policy granted 10% additional points on the entrance exam score of PHSS students applying to UFMG. Moreover, an additional 5% bonus was given to PHSS students who identified themselves as Blacks. Comparing students who enrolled in the university before and after the policy adoption, the authors show that the share of Blacks and PHSS in each academic program was statistically lower than the share of applicants from each of those groups before AAP. After the policy was implemented, the shares of Blacks and PHSS enrolled at UFMG became equivalent to the shares of applicants.

More recently, Estevan, Gall, & Morin (2016) investigated the adoption of a similar system of bonus at UNICAMP. The study followed university applicants before and after the policy adoption. Their results show a substantial increase in the enrollment of PHSS and students from lower-income households. However, although the policy included a specific bonus for Blacks, the enrollment of students from that group did not change significantly. Moreover, similarly to Card & Krueger (2005), the authors do not observe additional behavioral adjustments of applicants due to the policy, neither in terms of *vestibular* performance nor in terms of application decisions.

[38] To become eligible for the system of quotas, students were required to self-identify their race. However, UnB was one of the few universities who introduced a verification system on top of that declaration. Candidates selected for admission under the system of quota were analyzed by a university commission, which was supposed to confirm the race of the candidate. This commission got national attention from the media when in 2007 a pair of identical twins were differently classified, one as *Black* and the other as *White* (G1, 2007).

Going beyond the evaluation of a single university experience, Lopes (2016) investigated the distribution of AAP beneficiaries across different academic majors in Brazilian public universities. The study indicates that policy beneficiaries were generally enrolled in lower-prestige programs (defined in terms of average post-graduation earnings). Although the paper investigates a large set of universities, the analysis is restricted to cross-sectional observations of academic programs. Therefore, the author did not evaluate the impacts of AAP in the selection of students from AAP targeted groups.

To the extent of our knowledge, no other study has yet analyzed the overall impacts of the introduction of AAPs in all Brazilian federal universities. The examination of individual university experiences is important for understanding in-depth the specificities of each case. However, the results from these evaluations may be limited because they do not account for unobserved factors that may be concomitant with the adoption of AAP in each university. For example, the fact that UERJ observed a saturation of applicants from disadvantaged groups may not be completely explained by the university adoption of AAP as suggested by Cicalo (2012). Instead, the expansion of private universities and the government programs to finance tuitions and fees in those institutions may have also played an important role in reducing the demand for applications to some programs at UERJ. By comparing the profile of students admitted to universities that adopted AAPs with those admitted to universities that did not, we are able to account for time-specific unobservables that could confound the analyses based solely on an individual university's policy experience.

Moreover, by analyzing universities that adopted policies with different target groups, we are able to compare the outcomes of distinct types of AAP. One of the most controversial aspects of AAPs is the provision of a benefit based on race or skin color of individuals. Therefore, understanding the differences in outcomes between race-conscious and race-blind AAPs is extremely important to inform the policy debate. In the period we analyze in our study, 34 universities adopted some type of AAP. However, only 20 of these universities included race as an eligibility criterion for their policy.[39] The remaining 14 universities adopted AAPs that defined

[39] Out of these 20 universities, 17 had included both PHSSs and Blacks as AAP beneficiaries, and only 3 universities adopted AAPs targeting exclusively Black students

all PHSSs as beneficiaries, regardless of their race or ethnicity. Therefore, while we can classify the first group as race-conscious policies, the second group we classify as race-blind AAPs.

The fact that most Brazilian Universities targeted PHSS within their AAPs can be explained by the institutional segregation of students in Brazilian primary and secondary education. Public schools are free of charge at all levels of education; however, the quality of public primary and secondary schools is on average inferior when compared to their private counterparts. Moreover, enrollment in private secondary schools is strongly associated with higher income and socioeconomic status of households. Therefore, the type of secondary education is an easily identifiable indicator of lower socioeconomic background in Brazil, which is unlikely to be manipulated and is less subject to the controversies associated with racial identification and race-based favoring.[40] However, the statistical overlap between race and income may not guarantee that race-blind policies could be as effective as race-conscious policies in terms of improving the access of racial minorities. For instance, Darity, Deshpande and Weisskopf (2011) argue that class-based AAPs are inherently less effective than group-based policies to improve the access of discriminated groups, particularly when access to opportunities is based on some type of performance. However, the magnitude of this policy differential in the case of Brazilian colleges is ultimately an empirical question. Therefore, we take advantage of the fact that AAPs in our dataset were heterogeneous in terms of eligibility criteria to investigate whether there were any significant differences between the outcomes of race-blind and race-conscious policies, particularly with respect to the enrollment of racial minorities.

2.2 Data

2.2.1 Timeline of AAP adoption in Brazilian Federal Universities

Before the approval of the Law of Quotas in 2012, Brazilian federal universities had flexibility to define their own set of admission policies. In 2004, UNB became the first federal university to introduce an AAP for selecting its students, and in the following years other Brazilian

[40] For a detailed review of this topic, we refer to Daflon, Júnior, & Campos (2013).

universities adopted their own set of AAPs. To identify the moment of adoption and the exact policies implemented by each university, we collected information from university councils' minutes and admission process notices[41] from the period of 2004 to 2012.[42]

From these documents, we constructed the timeline of AAP adoption by Brazilian federal universities, which is presented in Table 2.1. Moreover, we were also able to identify the exact admission rules adopted by each institution. Appendix A details the admission policies of each federal University included in our study, and Table 2.2 summarizes the heterogeneity of policies with respect to their target groups, indicating the total number of universities that adopted *Race-Blind* or *Race-Conscious* AAP in the period.

2.2.2 ENADE

For the period covered in our analysis, there is not an available database with the characteristics of students enrolled in Brazilian public universities. Therefore, to identify the effects of AAP, we explore data from ENADE, a yearly exam conducted by the Brazilian National Government which is mandatory for students matriculated at federal universities.[43] The exam was created in 2004 with the objective of accessing the quality of tertiary education in the country. Students taking ENADE are required to fill out a socioeconomic form that includes questions about household socioeconomic characteristics and students' educational background. We use this self-reported information to tabulate the profile of students enrolled in the academic programs of Brazilian federal universities in the period of our analysis. The main variables used in our study include the race of students, household income, parents' education, gender, whether the student attended a public or private secondary school, and scores on the ENADE exam.

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[41] In Portuguese, these documents are referred as "*Editais de Chamada dos Processos Vestibulares*".

[42] The documents used to construct our timeline of affirmative action adoption by Brazilian federal universities are available at: http://rsvieira.com/projects/AA/timeline_documents/AA_adoption_documents.zip

[43] State universities are not required to participate in ENADE, although most of them join it voluntarily. A notable exception is the University of São Paulo, Brazilian largest public university, which did not participate in the exam until 2016.

Table 2.1: Brazilian Federal Universities by Year of AAP Adoption (2004-2013)

University	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
UnB	x	x	x	x	x	x	x	x	x	x
UFAL		x	x	x	x	x	x	x	x	x
UFBA		x	x	x	x	x	x	x	x	x
UFPE		x	x	x	x	x	x	x	x	x
UFPR		x	x	x	x	x	x	x	x	x
UFRPE		x	x	x	x	x	x	x	x	x
UFT		x	x	x	x	x	x	x	x	x
Unifesp		x	x	x	x	x	x	x	x	x
UFJF			x	x	x	x	x	x	x	x
UFPA			x	x	x	x	x	x	x	x
UFABC				x	x	x	x	x	x	x
UFMA				x	x	x	x	x	x	x
UFPI				x	x	x	x	x	x	x
UFRN				x	x	x	x	x	x	x
UFES					x	x	x	x	x	x
UFF					x	x	x	x	x	x
Ufop					x	x	x	x	x	x
UFRGS					x	x	x	x	x	x
UFSC					x	x	x	x	x	x
UFSCar					x	x	x	x	x	x
UFSM					x	x	x	x	x	x
UTFPR					x	x	x	x	x	x
UFG						x	x	x	x	x
UFMG						x	x	x	x	x
UFTM						x	x	x	x	x
UFRRJ							x	x	x	x
UFS							x	x	x	x
UFSJ							x	x	x	x
Univasf							x	x	x	x
URG							x	x	x	x
UFPB								x	x	x
UFRJ								x	x	x
UFU								x	x	x
UFMT									x	x
UFAC										x
UFAM										x
UFC										x
UFCG										x
Ufla										x
UFMS										x
UFPEL										x
UFRR										x
UFV										x
Unifal										x
UNIFAP										x
Unifei										x
Unir										x
Unirio										x

Table 2.2: Types of APPs Adopted by Brazilian Federal Universities (2004-2010)

	Universities	Ratio
Federal Universities	48	100.0%
Adopted AAPs	34	70.8%
Race-Blind Policies	14	29.2%
Race-Consious Policies	20	41.7%
Without AAPs	14	29.2%

Our main goal is to identify the characteristics of students enrolled in each academic program and how they changed after the adoption of AAP. To answer this question, we restrict the total sample of students who took ENADE to freshmen from federal universities in academic programs that were observed in at least two different rounds of ENADE. Until 2010, the exam was taken both by students in the first and final years of selected academic programs. However, in 2011, the examination of freshmen was discontinued, so our analysis is restricted to the period of 2004 to 2010. Hence, our final sample includes 170,555 freshmen students who took the ENADE exam between 2004 and 2010. These students were enrolled in 965 academic programs from 47 different federal universities,[44] and the median cohort in our sample had 54 students. Table 2.3 presents additional descriptive statistics of the sample of students included in our analysis.

It is worth noticing that the ENADE exam does not include all academic programs every year; instead, the exam is divided in cycles of three years. Academic majors[45] are divided into three different groups, and each group is evaluated every three years.[46] Therefore, because the sample of freshmen is restricted to the period of 2004 to 2010, we can only observe each university-program in either two or three rounds of the exam. Table 2.4 shows the academic majors included in each round of ENADE between 2004 and 2010, indicating the number of freshmen students

[44] UFABC only started participating at ENADE in 2011 http://www.ufabc.edu.br/noticias/alunos-da-ufabc-farao-a-prova-do-enade-pela-1o-vez.

[45] In Brazilian universities, students select their majors when they apply to the university. In most cases, if a student decides to change major after starting a program, he or she needs to retake the university entrance exam.

[46] For example, medicine programs were evaluated in 2004, 2007 and 2010.

from federal universities taking the exam every year.[47] In the next section, we detail our empirical model, presenting the main results and addressing potential limitations of our analysis.

Table 2.3: Descriptive Statistics – Freshmen from Federal Universities (ENADE 2004-2010)

	obs.	share[a]		obs.	share[a]
Universities	48	100%			
Programs	965	100%			
median cohort size	54	-			
Students	170,555	100%	***Students***	170,555	100%
Race			*Mother Education*		
Branco	63,136	60.3%	None	2,346	2.2%
Preto	6,653	6.4%	4th grade	13,885	13.3%
Pardo	32,160	30.7%	8th grade	12,485	11.9%
Amarelo	1,677	1.6%	Secondary	36,729	35.1%
Indígena	1,141	1.1%	Higher	39,260	37.5%
Black[b]	38,813	37.0%	NA[c]	65,850	38.6%
NA[c]	65,788	38.6%			
Type of High School			*Sex*		
All public	41,989	40.0%	Female	88,418	51.8%
Partial	11,114	10.6%	Male	82,137	48.2%
All private	51,741	49.4%	NA[c]	0	0.0%
NA[c]	65,711	38.5%			

Notes: [a] Shares are calculated excluding observations with no information available (NAs)
[b] Blacks include the sum of *Pretos* and *Pardos*
[c] NA shares are calculated by dividing Nas by total number of observations

[47] Appendix B shows the total number of observations by year and by federal university.

Table 2.4: ENADE's Sample of Freshmen from Federal Universities by Academic Major (2004-2010)

Major	**2004**	**2005**	**2006**	**2007**	**2008**	**2009**	**2010**	**Total**
Group 1								
Medicine	1,544	-	-	1,527	-	-	4,821	7,879
Nursing	1,202	-	-	1,595	-	-	3,658	6,511
Agronomy	1,315	-	-	1,836	-	-	3,774	6,558
Pharmacy	1,083	-	-	1,073	-	-	2,758	5,139
Social Services	1,097	-	-	1,054	-	-	2,573	5,038
Veterinary	710	-	-	1,198	-	-	2,440	4,192
Dentistry	787	-	-	948	-	-	2,270	4,084
Nutrition	678	-	-	904	-	-	1,820	3,635
Zootechnics	610	-	-	1,251	-	-	1,771	3,560
Kinesiology	1,382	-	-	1,610	-	-	1,407	4,730
Physiotherapy	296	-	-	293	-	-	776	1,774
Speech Therapy	157	-	-	119	-	-	259	789
Group 2								
Engineering	-	8,052	-	-	13,127	-	-	20,666
Pedagogy	-	2,403	-	-	4,396	-	-	7,009
Language	-	3,369	-	-	4,033	-	-	7,333
Biology	-	1,603	-	-	3,922	-	-	5,464
Mathematics	-	2,122	-	-	3,262	-	-	5,543
Chemistry	-	1,495	-	-	2,977	-	-	4,502
Computer Science	-	1,583	-	-	2,631	-	-	4,335
Physics	-	1,674	-	-	2,683	-	-	4,317
History	-	1,934	-	-	2,483	-	-	4,414
Geography	-	1,190	-	-	1,844	-	-	3,380
Social Sciences	-	1,182	-	-	1,635	-	-	2,952
Philosophy	-	756	-	-	996	-	-	2,076
Architecture	-	510	-	-	1,005	-	-	1,665
Group 3								
Business	-	-	2,633	-	-	8,207	-	11,098
Law	-	-	2,245	-	-	5,953	-	8,289
Economics	-	-	2,016	-	-	4,195	-	6,452
Accounting	-	-	1,773	-	-	4,305	-	5,959
Social Communication	-	-	1,674	-	-	2,530	-	4,327
Psychology	-	-	1,261	-	-	2,317	-	3,604
Music	-	-	742	-	-	1,720	-	2,909
Library science	-	-	1,134	-	-	1,731	-	2,987
Tourism	-	-	605	-	-	964	-	1,741
Design	-	-	513	-	-	1,011	-	1,687
Dramaturgy	-	-	536	-	-	812	-	1,533
Executive Secretariat	-	-	218	-	-	271	-	594
Other								
Biomedicine	-	-	279	276	-	-	616	1,257

* Does not include tecnology programs

2.3 Empirical Model and Results

The goal of our analysis is to identify how AAPs changed the characteristics of students enrolled in academic programs. The key for our identification strategy is the time heterogeneity in the adoption of AAP in each Brazilian federal university in the period of our analysis. Using a differences-in-differences strategy, we compute the changes in the average characteristics of freshmen students in each academic program of our sample. The policy effects are then calculated as the average difference in those changes between programs from universities that adopted AAP and those that did not adopt any policy.

2.3.1 Baseline Model

We start our empirical analysis by defining a baseline model that estimates the average policy effect in all programs that adopted AAP in the period. However, because each academic program is only observed once every three years, we separate observations into two cycles of three years each. The first cycle includes students who took the exam in 2005, 2006 and 2007,[48] and the second cycle includes observations from 2008, 2009 and 2010. With this setup, each academic program is observed twice and our baseline empirical model can be described by the following equation:

$$y_{pt} = \alpha_p + \left(\beta + \gamma\, ET_p + \delta\, LT_p\right) \cdot T_t + \varepsilon_{pt} \qquad (1)$$

Where y_{pt} is either the log of the share of students with a certain characteristic – Black, public high-school, parents with primary education attainment, or female – or the average normalized ENADE score of each university-program[49] cohort p. The coefficient α_p is a university-program fixed effect that does not vary on time, and it captures the average characteristics of students in each program in the first cycle. The variable $Post_t$ is an indicator for

[48] In the baseline model, we exclude the observations from 2004. By doing that, all academic programs are observed in the same number of periods. If instead, we had part of the observations with three periods and part with two, we would need to estimate an additional set of coefficients that would be specific for the former group, hindering the interpretation of results. In Session III.B we use the group of programs observed in three different years to test for pre-treatment parallel trends.

[49] A university program is defined as an academic major from a specific university. For example, the program of Law at UnB.

observations from the second cycle of exams, that is, it takes the value of 1 for observations of students who took the exam in 2008, 2009 and 2010, and takes the value of zero otherwise:

$$\begin{aligned} Post_t &= 0 \quad if \quad t \in (2005, 2006, 2007) \\ Post_t &= 1 \quad if \quad t \in (2008, 2009, 2010) \end{aligned} \tag{2}$$

Therefore, the coefficients β, γ and δ compose the average changes of student characteristics when comparing the second to the first cycle. While β is the average change observed throughout all academic programs, γ is specific for programs that adopted AAP in the first cycle, that is, the Early Treatment (ET) group. Similarly, δ is the equivalent coefficient for the Late Treatment (LT) group, which are the programs which had no AAP during the first cycle but had adopted it in the second cycle. Programs without any AAP in neither the first or second cycles are defined as Controls (Co). Therefore, the average change of student characteristics is calculated by different coefficient compositions for each group of programs. While for the Co group the average change is given by β, for the ET group it is given by $\beta + \gamma$ and for the LT group it is given by $\beta + \delta$. Therefore, the coefficient δ can be interpreted as the average treatment effect of AAP adoption if we assume that β corresponds to the counterfactual change that would be expected if the Late Treatment universities had not adopted any AAP.[50]

As for the dependent variables used in our models, we investigate a series of different student characteristics that include: race, type of secondary school, socioeconomic background, gender, and the score at the ENADE exam. In the case of race, our main variable is an indicator of Blacks, which is the pooling of students who self-classify as either *Preto* or *Pardo*. For the type of secondary school, we focus on students who report completing all of their secondary education in public-schools.

With respect to the socioeconomic background, we define as an indicator of lower-strata the students who report that their parents have not studied beyond primary education.[51] It is well established in the economic literature that education attainment is closely associated with income and wealth.

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[50] Similarly, the coefficient γ can be interpreted as a lagged effect of AAP on the Early Treatment group.

[51] That is, the indicator only takes a value of one if neither the mother nor the father have studied beyond primary education. 20.8% of students in our sample have both parents with no more than primary education attainment.

We also analyze the share of female students in each program to verify whether the expansion of AAP was associated with any indirect effect in the enrollment of women in tertiary education.[52] And finally, we also evaluate changes in average ENADE scores of students in each program.[53]

Table 2.5 shows the regression results for this baseline model specification where the treatment effect is pooled across all academic careers and types of AAP. The main coefficient of interest in each regression is the interaction between observations from the second period and late treatment universities ($Late\ Treatment \times\ Post$), which corresponds to coefficient δ in Equation 2. These coefficients can be interpreted as the average changes in the dependent variables for universities that adopted AAP compared to the changes observed in the control group. The results show that the share of Blacks, PHSS and Low-Education Parents increased significantly more for programs that adopted AAPs compared to the control group. The increase in the enrollment of Blacks was 8.8p.p. higher on treated universities if compared to the average change observed on the control programs (6.5%). Meanwhile the increase in the share of PHSS was 16.4p.p., and students with low-education 12.2p.p. above the respective increases on the controls. These results confirm that, on average, the groups targeted by the AAPs had a positive and significant enrollment gain in treated programs if compared to programs from universities that did not adopt any type of AAP.

No significant effects were observed associating the adoption of AAP with the share of women or the average ENADE score of students. A major criticism of AAP is that it could lead to the selection of poorly prepared students, causing a reduction in the overall quality of programs. However, in line with most of the empirical literature investigating this question, we do not observe any significant changes in the ENADE score of students from universities after they adopted AAP. Although ENADE is limited as a measure of academic abilities, an interesting aspect of our result is that all students included in our sample are freshmen, so the similarity in performance between

[52] Bertrand, Hanna, & Mullainathan (2010) reported that caste-based AAPs in India led to a reduction in the overall number of females entering engineering colleges.

[53] While we are aware of the limitations of the ENADE score as a measure of student abilities, we include this estimation in our analysis so we can compare our results with previous findings in the literature of AAP mismatch hypothesis. Only for this variable, the dependent variable cannot be calculated as the log of the share per cohort, so instead we compute the normalized average score per cohort.

treated and untreated groups cannot be explained by AAP beneficiaries catching up to non-beneficiaries during the college years.

As for the other coefficients estimated by our model, the results indicate that for the Early Treatment group, the share of PHSS also grew by approximately 18.2% during the second period of our analysis, a result that contradicts the concern of a possible saturation in the demand for public higher education from disadvantaged students as raised by Cicalo (2008), at least in the initial years after the adoption of AAP.

Table 2.5: OLS Regression - Changes in The Profile of Enrolled Students by AAP Adoption

	Dependent variable:				
	Black[a]	PHSS[b]	Low-educ parents[c]	women	ENADE score[d]
Late AAP Adopter[e] × Post[g]	0.088 **	0.164 ***	0.122 *	0.014	-0.001
	(0.031)	(0.035)	(0.049)	(0.019)	(0.037)
Early AAP Adopter[f] × Post	-0.002	0.182 ***	0.080	-0.025	0.060
	(0.033)	(0.038)	(0.053)	(0.021)	(0.041)
Post	0.065 **	0.070 **	-0.007	0.065 ***	-0.002
	(0.021)	(0.024)	(0.033)	(0.013)	(0.028)
Program FE	yes	yes	yes	yes	yes
Observations	1,900	1,944	1,760	2,032	730
Adjusted R^2	0.855	0.734	0.757	0.888	0.419

notes: *p<0.05, **p<0.01, ***p<0.001.

Coefficients can be interpreted as relative changes. E.g., a coefficient of 0.5 indicates an increase of 50%

[a] Black: refers to the combined group of Pretos and Pardos

[b] PHSS: Public High School Student (all years)

[c] Low-educ parents: none of the student's parents have studied beyond primary education

[d] ENADE score: standard deviations to the mean score of freshmen from federal university in the same major

[e] Late AAP Adopter: Programs which had no AAP in 2005-2007, but adopted it in 2008-2010

[f] Early AAP Adopter: Programs which adopted AAP in 2005-2007

[g] Post: dummy indicating the second cycle of ENADE exams (2008, 2009 and 2010)

Finally, it is also interesting to notice the overall changes of student characteristics which are described by the coefficient for $Post$ only. These results indicate positive and significant average changes in the shares of Blacks, PHSSs and women, even on programs where AAP was not adopted. These results highlight the importance of comparing the changes of programs that adopted AAP with other programs in order to avoid overestimating the treatment effects of AAP.

2.3.2 Heterogeneous Effect by Program Competitiveness

An important question about the effectiveness of AAPs relates to the heterogeneity of the policy impact with respect to programs' prestige. The literature based on the American experience indicates that AAP effects are usually restricted to top tier institutions. Meanwhile the policies seem to have negligible effects in less prestigious programs (Epple, Romano, & Sieg, 2008), (Long, 2004), (Arcidiacono, 2005), Hinrichs (2012), (Backes, 2012). However, with respect to the Brazilian experience, the existing results from the literature are not yet clear. While AAP beneficiaries seem to be more concentrated in less prestigious academic programs (Cicalò, 2008), (Lopes, 2016), there is yet no evidence on how the expansion of AAPs has changed the enrollment of disadvantaged students in academic programs with different levels of competitiveness for admission.

To investigate this question, we estimate an extended version of our baseline model where we interact the treatment effect of AAP with a measure of program competitiveness (C_{pj}):

$$y_{pt} = \alpha_p + \sum_j \left((\beta_j + \gamma_j ET_p + \delta_j LT_p) \cdot T_t \cdot C_{pj} \right) + \varepsilon_{pt} \tag{3}$$

Differently from the baseline model, the coefficients β, γ and δ are disaggregated for programs with different levels of competitiveness j. It is important to notice that we define the competitiveness of each program based on their corresponding minimum score for general admission in 2016.[54] Based on this metric, we separate programs into three categories: low-,

[54] The minimum score for admission for each program was taken from SISU, which is a unified system of student admission for Brazilian universities that was established in 2010. We use the SISU cutoff score from 2016 for defining program competitiveness for two main reasons: 1) although the system was created in 2010, the adoption of SISU by federal universities was gradual, therefore the match between the programs participating in SISU and the programs in the ENADE dataset is higher for the most recent available year. 2) The minimum SISU score for each program before 2016 could be associated with the

medium- and high-competitiveness.[55] We divide the programs into 3 quantiles, so the cutoff scores for each group are defined at 645 and 700 points, leading the indicator variables C_{pj} to be defined as:

$$
\begin{aligned}
C_{p,low} &= 1 \quad if \quad SISU_p < 645 \\
C_{p,low} &= 0 \quad otherwise \\[1em]
C_{p,med} &= 1 \quad if \quad 645 < SISU_p < 700 \\
C_{p,med} &= 0 \quad otherwise \\[1em]
C_{p,hig} &= 1 \quad if \quad 700 < SISU_p \\
C_{p,hig} &= 0 \quad otherwise
\end{aligned}
\tag{4}
$$

Figure 2.1 shows the histogram of the SISU 2016 minimum score for the academic programs included in our analysis and how they relate to the competitiveness categories we have just defined. Table 2.6 shows the results for the regressions where we interacted the original treatment groups with an indicator of competitiveness for each academic program as described by Equation 3:

Overall, the results indicate that the effects of AAP adoption were larger and mostly restricted to the more competitive programs. In the group of programs identified as highly-competitive, the shares of Blacks, PHSS, and students with low-educated parents increased respectively by 19.6, 39.7 and 30.5 percentage points above the corresponding changes on the control programs. Meanwhile, the average changes on low-competitive programs were roughly negligible and statistically not different from zero. In the case of medium-competitive programs, point estimates were slightly larger, but still not significant.

Additionally, we have not observed any significant association between the adoption of AAP and changes in the gender composition of cohorts in none of the competitiveness groups included in our analysis.

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adoption of AAPs before the Law of Quotas, hence confounding competitiveness with different levels of quotas. However, by 2016, the AAP rules of all federal universities were mostly homogenized due to the Law of Quotas.

[55] Appendix F presents the competitiveness model estimation results using alternative definitions of program competitiveness groups. While we lose precision by adding additional competitiveness groups, the overall results remain mostly the same, with larger effects observed on programs with higher SISU 2016 scores.

Table 2.6: OLS Regression - Changes in the Characteristics of Enrolled Students by AAP Adoption and Program Competitiveness

	Dependent variable:				
	Black[a]	PHSS[b]	Low-educ parents[c]	women	ENADE score[d]
Low-Compet.[g] × LT AAP[e] × Post[f]	0.021 (0.079)	0.017 (0.092)	0.081 (0.120)	-0.049 (0.048)	0.227 (0.141)
Medium-Compet. × LT AAP × Post	0.106 (0.060)	0.044 (0.069)	0.009 (0.093)	0.051 (0.037)	-0.040 (0.083)
High-Compet. × LT AAP × Post	0.196 *** (0.057)	0.392 *** (0.066)	0.305 *** (0.092)	0.016 (0.036)	0.024 (0.059)
Program FE	yes	yes	yes	yes	yes
Observations	1,384	1,412	1,294	1,476	456
Adjusted R^2	0.871	0.759	0.776	0.900	0.397

notes: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

Coefficients can be interpreted as relative changes. E.g., a coefficient of 0.5 indicates an increase of 50%

For consiseness, we ommit the coefficients for the interactions between competitiveness, ET and Co programs.

Appendix C reports the whole set of coefficients estimated from this model.

[a,b,c,d,e,f] : see notes from Table 5;

[g] Program competitivenss was defined based on the minimum SISU score of 2016. For details, see Figure 1.

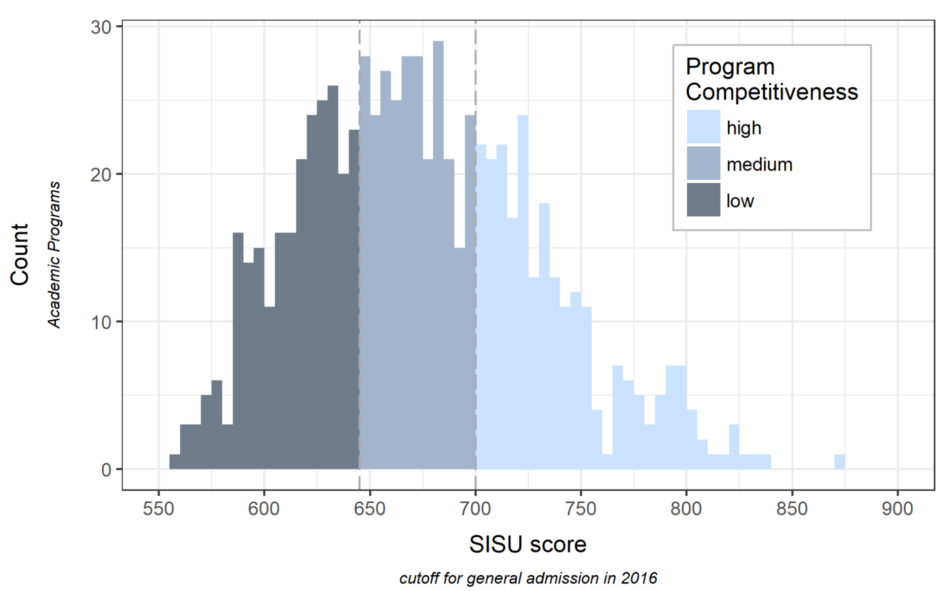

Figure 2.1: Histogram of Minimum SISU Score for General Admission in 2016 Federal University Programs included in the ENADE exams of 2004-2010

Once again, we found no significant effects of the adoption of AAP on the performance of students at the ENADE exam, even in the case of highly-competitive programs. This result is particularly strong against the mismatch hypothesis since these are the programs where one would expect a larger gap between beneficiaries and their peers. Although we believe this is an interesting result, it should be viewed with caution due to the limitations of ENADE as a metric for students' performance. Further research with better metrics would be required for a properly examining this question.

In synthesis, the results from this analysis indicate that the effects of AAPs on improving the enrollment of students from disadvantaged backgrounds in Brazil were mostly concentrated on the high-competitive programs, a result that is in line with what was previously found in the American experiences with AAP.

2.3.3 Race-Blind vs Race-Conscious Policies

We now investigate the effectiveness of race-blind policies to indirectly improve the access of racially discriminated groups to college in Brazil. We classify the AAPs adopted by the universities in our sample as race-blind (RB) if they do not include race or ethnicity as an eligibility criterion and race-conscious (RC) if they do. We then estimate the following extension of our baseline model:

$$y_{pt} = \alpha_p + (\beta + \gamma_{RB}\, ET\_RB_p + \gamma_{RC}\, ET\_RC_p + \delta_{RB}\, LT\_RB_p + \delta_{RC}\, LT\_RC_p) \cdot T_t + \varepsilon_{pt} \quad (4)$$

Here, each treatment group (ET and LT) are further divided into race-blind (RB) or race-conscious (RC). The interpretation of coefficients mostly mirrors the baseline model. However, within this specification, the main question is the existence of differences between δ_{RC} and δ_{RB}, that is, the LT specific average changes in the profile of students after the adoption of AAP, particularly in the case of dependent variables associated with racial characteristics.

Table 2.7: OLS Regression - Changes in the Characteristics of Enrolled Students by AAP Adoption and by Type of AAP (Race-Blind vs. Race-Conscious)

	Dependent variable:				
	Black[a]	PHSS[b]	Low-educ	women	ENADE
A: All Programs					
Race Blind[g] AAP LT[e] × Post[f]	-0.028	0.122 *	-0.010	0.020	-0.017
	(0.041)	(0.048)	(0.065)	(0.025)	(0.056)
Race Conscious[h] AAP LT × Post	0.169 ***	0.191 ***	0.214 ***	0.010	0.004
	(0.036)	(0.041)	(0.057)	(0.022)	(0.040)
Post	0.065 **	0.070 **	-0.007	0.065 ***	-0.002
	(0.021)	(0.024)	(0.033)	(0.013)	(0.028)
Program FE	yes	yes	yes	yes	yes
Observations	1,900	1,944	1,760	2,032	730
Adjusted R^2	0.858	0.734	0.761	0.889	0.416
B: High-Competitive Programs Only					
Race Blind AAP LT × Post	0.035	0.217 *	0.162	0.040	-0.057
	(0.092)	(0.102)	(0.141)	(0.047)	(0.102)
Race Conscious AAP LT × Post	0.271 ***	0.475 ***	0.376 **	0.005	0.047
	(0.074)	(0.082)	(0.116)	(0.038)	(0.066)
Post	-0.004	-0.184 **	-0.263 **	0.044	-0.008
	(0.052)	(0.058)	(0.084)	(0.027)	(0.046)
Program FE	yes	yes	yes	yes	yes
Observations	438	444	372	470	212
Adjusted R^2	0.873	0.767	0.740	0.926	0.461

notes: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

Coefficients can be interpreted as relative changes. E.g., a coefficient of 0.5 indicates an increase of 50%

For concieness, we ommit the coefficients associated with early treatment programs.

Appendix C reports the whole set of coefficients estimated in this model.

[a,b,c,d,e,f]: see notes from Table 5;

[g] Race Blind: AAPs without any race specific eligibility criteria. In all of or cases eligibility was granted to PHSS

[h] Race Conscious: AAPs with race specific eligibility criteria. Either exclusive or combined with PHSS status

Table 2.7 presents the results of this estimation. Given that we've found the effects of AAPs to be mostly concentrated on highly competitive programs, we estimate this model with two sets of data, A) pooling all programs from our dataset; B) restricting the sample to the highly-competitive programs only.

The results indicate that race conscious policies were associated with a larger increase of Blacks, PHSS and students with low-educated parents if compared to race blind policies. Not only the results were larger, but in the case of the enrolment of Blacks and of people whose parents had low-education attainment, the outcomes of race blind AAPs were not statistically different from zero. Even in the case we restrict the analysis to high-competitive programs, the results are still the same, that is, race conscious policies are associated with an increase of all types of disadvantaged groups, while race blind results are limited to PHSS, with negligible effects in the enrollment of Blacks.

These results suggest an overall ineffectiveness of race blind policies to indirectly increase the enrollment of racial minorities. Although income, type of school and race are strongly interconnected attributes in Brazil, AAPs using high-school type as the sole eligibility criteria have had no effects on improving the enrollment of Blacks, at least in the programs evaluated in our sample. On the other hand, universities that adopted policies with explicit race-conscious criteria significantly increased their shares of Black students. Moreover, in the case of race-conscious policies, we also observed an increase in the enrollment of students whose parents have not completed beyond primary education, indicating that race-conscious AAPs were also effective in improving the enrollment for of the least wealth individuals of society.

2.3.4 Empirical Model Limitations and Possibly Confounding Factors

The main underlying assumption for the validity of our estimates as the causal effect of AAP is that in the absence of the policy implementation, the changes that would occur on the treated group would be equivalent to changes observed on control universities. Therefore, there are two main threats to the validity of our estimates of the effects of AAPs: 1) the existence of pre-treatment trend differences between universities that adopted or not AAPs; 2) unobserved shocks associated with the adoption of AAPs that may have also affected the enrolled of disadvantaged students in each program. In what follows we examine in further details each of these threats.

2.3.4.1 Pre-treatment trends

In the case of differences in pre-treatment trends, we could empirically test it if we had at least two observations for each academic program before the adoption of AAP by the treatment group. Therefore, in our setting, such data is only available in the case of the academic majors included in the Group 1 of ENADE. These programs were evaluated in 2004, 2007 and 2010. Therefore, for this group it is possible to analyze, for each dependent variable, whether paths were parallel between 2004 and 2007 for the Control and the Late Treatment groups, that is, universities that did not adopt AAP and those which adopted it between 2008 and 2010. To conduct this test, we subset our sample to Group 1 of academic majors and estimate pre-treatment common trends using a model with fully flexible group-specific dynamics as described by Mora & Reggio (2017), which in our setting, is represented by Equation 5 below:

$$y_{pt} = \alpha_p + \left(\beta_{2007} + \gamma_{2007} ET_p + \delta_{2007} LT_p\right) \cdot T_t^{2007} + \left(\beta_{2010} + \gamma_{2010} ET_p + \delta_{2010} LT_p\right) \cdot T_t^{2010} + \varepsilon_{pt} \quad (5)$$

Differently from the baseline model, this estimation includes observations from three different periods, therefore the indicator variables of time-periods are defined as:

$$\begin{aligned} T_t^{2007} &= 1 \quad if \quad t = 2007 \\ T_t^{2007} &= 0 \quad otherwise \\ \\ T_t^{2010} &= 1 \quad if \quad t = 2010 \\ T_t^{2010} &= 0 \quad otherwise \end{aligned} \quad (6)$$

Again, programs from universities that adopted AAP in 2007 are classified as Early Treatment (ET), programs that adopted AAP in 2010 are classified as Late Treatment (LT), and programs without any AAP are classified as Controls.[56] If common trends existed between Late Treatment and Control groups before the adoption of AAP by the former group, then coefficient δ_{2007} should not be statistically different from zero for none of our dependent variables.

Table 2.8 reports the point-estimates of δ_{2007} for each of the dependent variables included in our analysis. All estimates are not statistically different from zero, therefore we cannot reject

[56] Within our sample, besides UNB, no other university had adopted AAP by 2004. Therefore, to avoid estimating a set of coefficients based on a single university, we exclude the observations of students from UNB from this analysis.

the assumption of common trends between treated and control universities, at least before the adoption of AAPs by the Late Treatment group. Figure 2.2 shows the results from this exercise graphically, where the segments between 2004 and 2007 indicate the pre-treatment trends – which are the ones we need to assume are equivalent between LT and Co groups – and the segments from 2007 and 2010 indicate the group specific trends after AAP adoption by the LT group.

Although the regression results of interest are not statistically significant, the point estimate are negative for the share of Blacks, PHSS and students with low-education parents. So, if anything, those shares were actually decreasing on universities that later adopted AAPs if compared to the control group. Therefore, these results strongly support the interpretation of our main results, that is, AAPs had a positive and significant effect on the enrollment of targeted groups.

Table 2.8: OLS Regression – Pre-Trend Differences between Late Treatment and Control Groups Before the Adoption of AAP (Group 1 of ENADE Majors)

	Dependent variable:				
	Black[a]	PHSS[b]	Low-educ parents[c]	women	ENADE score[d]
LT AAP × 2007 [e]	-0.099 (0.087)	-0.025 (0.096)	-0.122 (0.130)	0.011 (0.038)	0.046 (0.115)
Program FE	yes	yes	yes	yes	yes
Observations	660	666	564	705	126
Adjusted R^2	0.844	0.753	0.701	0.868	0.179

notes: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

Coefficients can be interpreted as relative changes. E.g., a coefficient of 0.5 indicates an increase of 50%

For consiseness, we ommit the coefficients for the interactions between 2007, 2010 and ET.

Appendix C reports the whole set of coefficients estimated from this model.

[a,b,c,d] : see notes from Table 5;

[e] LT AAP × 2007: Difference in trends between LT goup (Treated in 2010) and Control before treatment.

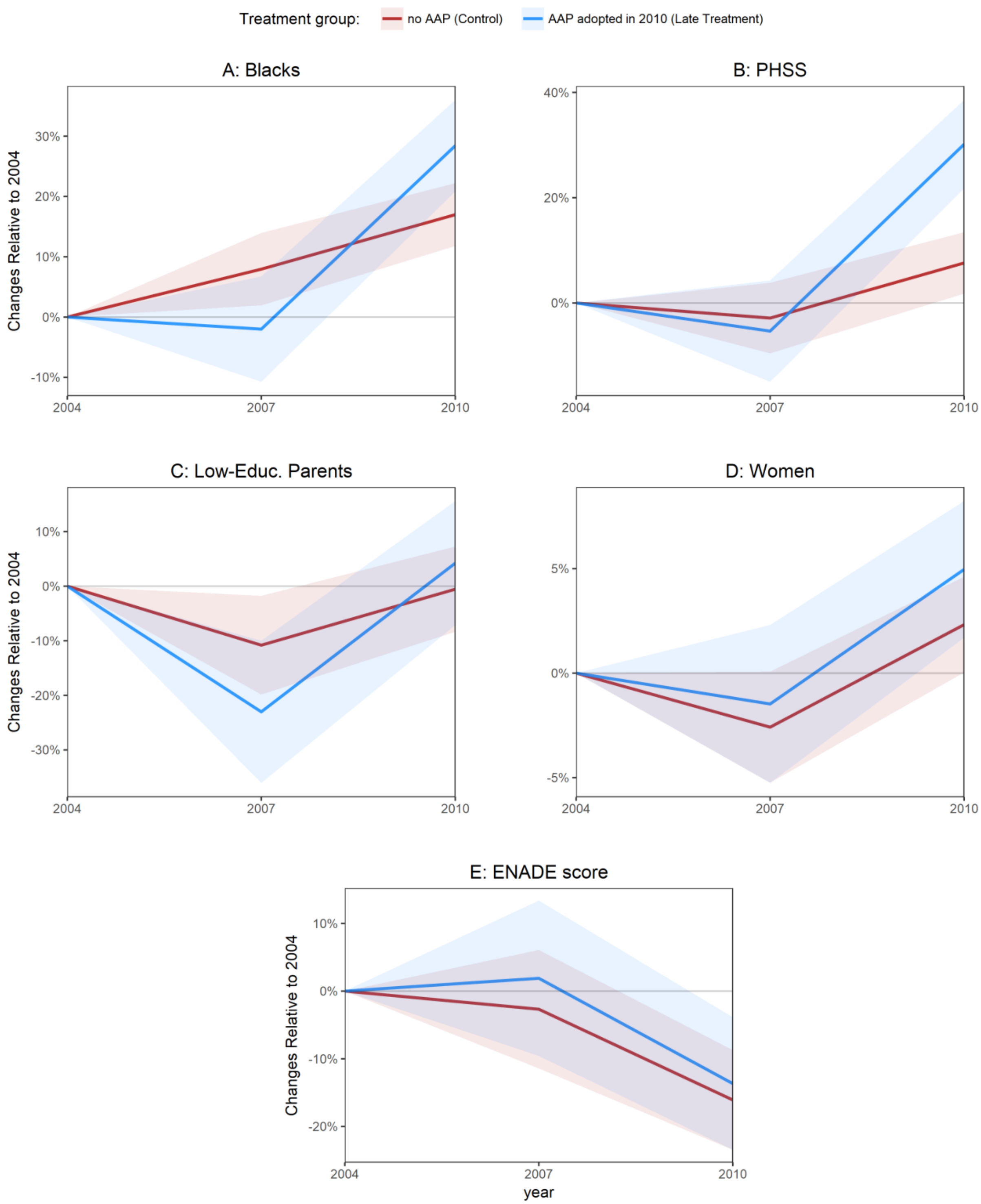

Figure 2.2: OLS Regression Results – Trends of Dependent Variables for Late Treatment and Control Groups (Group 1 of ENADE Majors)

2.3.4.2 *Possibly Contemporaneous Confounding Factors*

The second threat to the validity of our estimates are the possibility of unobserved shocks associated with the adoption of AAP by each university that could also affect the selection of disadvantaged students. When comparing public universities with respect to the adoption of AAP, Daflon, Júnior, & Campos (2013) find no significant differences in terms of geographical distribution[57] and academic ranking of universities. However, other factors with relevant impacts on the profile of enrolled students could still be correlated with the adoption of AAPs. To address this issue, we test the association between the adoption of AAP and five possibility confounding variables: 1) the expansion of undergraduate positions, 2) the expansion of nearby competing private universities, 3) the growth of regional economic activity, 4) the share of black students graduating from high school, and 5) the share of high school graduates from public high schools. For each of these variables, we estimate their association with the adoption of AAP through the following regression model:

$$\Delta z_{ut} = \phi \, AAP_{ut} + \mu_t + \varepsilon_{ut} \tag{7}$$

Where Δz_{ut} is the change in variable z_u between years t and $t-1$. Moreover, AAP_{ut} is an indicator if university u had adopted an AAP by year t. Finally, μ_t are year fixed effects. If the adoption of AAPs by federal universities were associated with any of the changes in potentially confounding variables z, then coefficient ϕ would capture that relationship.

Additionally, we also explore 3 additional potentially confounding factors that could be associated with the ENADE data sampling: 1) the cohort size, 2) students from the night session of each program, and 3) missing data from the socioeconomic questionnaire. For these variables, Equation 7 is extended to include Program Fixed Effects as we have data at the student level:

$$z_i = \phi \, AAP_{it} + \mu_t + \lambda_p + \varepsilon_i \tag{8}$$

[57] Appendix D shows the map with the location of Brazilian federal universities by AAP treatment status.

Table 2.9: OLS Regression - AAP Adoption and Possibly Confounding Variables

	Dependent variable:							
	Univ Expans	ΔPriv Univ	ΔGDP	ΔPHSS ENEM	ΔBlack ENEM	cohort size	night session	n.a. data
AAP	0.012	-0.002	-0.007	0.002	-0.015 **	16.7	-0.012	0.001
	(0.053)	(0.014)	(0.008)	(0.004)	(0.006)	(11.6)	(0.019)	(0.023)
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Program FE						yes	yes	yes
Obs	336	336	336	336	336	176,000	176,000	176,000
Adj R^2	0.086	0.144	0.101	0.459	0.698	0.908	0.502	0.115

notes: *p<0.1, **p<0.05, ***p<0.01. For student data, standard errors are clustered at the university level

Results for the estimation of Equations 7 and 8 are shown in Table 2.9. The association between the adoption of AAP and each of the possibly confounding variables we investigate seems to be mostly negligible. The only significant result is a reduction of approximately 1.5% in the share of Blacks taking ENEM in the states where universities adopted AAPs. Even though the magnitude of the association is small, the sign of the coefficient is in the opposite direction to the result that would confound our results. That is, the adoption of AAP does not seem to be associated with an increase in the shares of Black students graduating from secondary education or applying to college. If anything, this result indicates that the coefficients we estimate for the effects of AAPs adoption in the enrollment of Black students could be downward biased.

In conclusion, from the analysis carried in this section, we do not observe pre-treatment differences between universities that adopted or did not adopt AAPs with respect to our dependent variables. Additionally, we did not find evidence of any association between the adoption of AAP and variables that could potentially be associated with the selection of disadvantaged students.

2.4 Conclusion

This paper investigated the effects of the adoption of AAPs by Brazilian federal universities. Using a difference-in-differences design, we explored the heterogeneity in the time of policy adoption to identify the effects of AAP on the enrollment of students from disadvantaged groups. Our results indicate that the AAPs adopted in the period we analyzed were particularly effective in improving the enrollment of students from groups explicitly targeted by each policy. Most universities in our sample adopted rules to favor the selection of students who graduated from public high schools or who were self-identified as Blacks, and accordingly, we observed an overall increase in the enrollment of students from those groups.

However, the increase in the enrollment of Blacks was only observed in the academic programs from universities that adopted AAPs with explicit racial criteria. A common argument in the debate about affirmative action is that race-blind policies would be preferred because the interconnected relationship between race and socioeconomic conditions insure that, policies targeting socioeconomically deprived individuals would indirectly benefit racial minorities without relying on controversial race-based preferences. However, we have shown that race-blind AAPs have had a negligible effect on the enrollment of Blacks. Meanwhile, race-conscious policies not only were associated with a larger share of admitted Black students, but also had larger impacts on the enrollment of individuals with worse socioeconomic characteristics.

Similar to other results from the international literature, we also observed that the effects of AAPs were larger for more competitive programs, while they were mostly negligible for less-competitive ones. Moreover, we did not find any evidence of differences in the academic performance of students enrolled in programs that adopted AAP nor any differences in their gender composition.

We acknowledge that the main limitation of our study relates to the source of the information we have used to identify the characteristics of students in each program. The demographic data from the ENADE exam is limited because it is self-reported and academic programs are observed only once every three years. However, it is the best available source of information for the set of students enrolled in the academic programs of Brazilian Federal Universities in the period we analyzed in this paper. Linking the ENADE data with other sources of students' information is a suggested next step for improving the quality and precision of the analysis carried in this paper. Additionally, our analysis is constrained to a single aspect associated with the adoption of AAPs. Further questions on the impacts of these policies in the Brazilian context remain unanswered, and include: the graduation rates of beneficiaries, the impacts of college access on labor market earnings and the overall effects of the policy on economic inequality.

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Chapter 3: Travel Behavior Effects of Fare-Free Public Transportation for the Elderly in Brazilian Metropolitan Areas

This paper investigates how a policy that grants fare-free public transportation for the elderly affects the travel behavior of its beneficiaries. To identify the policy impacts, I explore the fact that eligibility for exemption is based on age thresholds that vary by gender and by city. Using data from household travel surveys, I estimate the causal effects of free-fare on travel behavior through a Regression Discontinuity Design. The results of my analysis indicate that eligibility for fare-free public transportation increases transit ridership among beneficiaries in approximately 27.3%. However, I do not find any significant effects of the policy on mode substitution nor on trip characteristics.

Keywords: Fare-Free Public Transportation, Seniors Free Ride

Subsidies to public transit fares are a common policy throughout the world. This type of price intervention can be economically justified as a second-best mechanism to attenuate the external costs associated with the use of private vehicles (Parry & Small, Should Urban Transit Subsidies Be Reduced?, 2009).[58] However, the effectiveness of such policy depends on the elasticity of substitution between private and public modes. Yet, empirical causal evidence about the magnitude of such elasticity is rather limited, particularly in the developing world. Exogenous price shocks, which are key for identifying the slope of the demand curve, are hardly available in these markets. This limitation of information is even more critical in the case of non-marginal price changes, which are often suggested as optimal fare policies such as in Parry & Small (2009) and Basso & Silva (2014).

This paper aims to address this gap by providing novel empirical evidence about the elasticity of substitution of transport modes given a non-marginal transit fare reduction. Specifically, I explore a Brazilian policy that grants fare-free public transportation to senior citizen.

[58] As a recent example of this argument, a complete subsidization of transit-fare has been proposed in different German cities with the objective of reducing emissions (The Washington Post, 2018).

Using a rich dataset of individuals' travel behavior, I compare the number of trips, by different modes, made by individuals who are just above and just below the fare exemption threshold age. Though this empirical strategy, I identify the causal impact of a complete fare subsidization on individuals' travel behavior.

The results of this analysis indicate that fare-free public transportation increases transit ridership of beneficiaries by approximately 27.3%. However, I do not identify any significant effects on mode substitution, particularly in the case of trips made by private vehicles.

Compared to the existing literature on the effects of transit fare subsidies, this study has three major differentials: First, my results are based on ex-post reduced form estimations of policy impacts. Most of the literature evaluating the effects of transit subsidies is based on elasticities estimated or inputted from structural models,[59] which tend to present more elastic demand curves if compared to empirical estimations from reduced form econometric models (Kremers, Nijkamp, & Rietveld, 2002). Second, I explore the effects of a non-marginal fare difference where individuals experience mostly identical conditions except for the transit fare. In the transportation literature, elasticities estimated from reduced form models are usually based on before-and-after comparisons.[60] However concomitant with fare price adjustments, other structural changes are usually implemented, thus possibly confounding the effects of price on demand. Third, my paper analyses the effect of complete fare subsidization in a developing world setting, where the mode share of public transit is much higher[61] than in developed world, and consequently public transit policies have a more extensive impact on welfare.

The results from my analysis may not be directly generalized to other contexts or other groups of individuals, however, understanding the outcomes of transit subsidization for the elderly in Brazil is an important question on its own. The Brazilian national constitution requires that urban public transportation should be free of charge to all elderly individuals. So, understanding how the policy affects its beneficiaries is important for policy makers in the country, especially considering the aging characteristic of the Brazilian population. Demographic projection indicates

[59] Some examples include: (Basso & Silva, 2014), (Tscharaktschiew & Hirte, 2012), (Proost & Dender, 2008), (Parry & Small, 2009)

[60] For example, (Cats, Susilo, & Reimal, 2016), (Rye & Mykura, 2009)

[61] In the Metropolitan Region of São Paulo, more than half of motorized trips are made by public modes, corresponding to more than 16 million trips per day (METRO, 2013).

that, by 2030, the share of trips made by individuals who are eligible for fare exemption will increase by 50.8% (Pereira, Carvalho, Souza, & Camarano, 2015). Therefore, the government expenditure for providing fare-free public transportation to the elderly is expected to increase substantially if the existing eligibility rules are not modified. Compelled by this scenario, policy makers are discussing reviewing the eligibility rules for fare exemption.[62] So, the results from this paper can also be used to inform this policy debate.

3.1 Background

3.1.1 Transit Fare Subsidies and the Elasticities of Substitution

Theoretically, subsidies to the transit fare can be supported as a welfare improving pricing strategy (Basso, Guevara, Gschwender, & Fuster, 2011). Among the rationales for intervening in transit the fare, we have that the marginal cost of driving private vehicles does not internalize the social costs associated with its use, which include pollution, accidents and congestion. Therefore, subsidizing public transit could act as a second-best solution for these externalities as it creates an incentive for drivers to substitute their trips to public modes.[63] Given this mechanism, models evaluating the optimality of transit fare subsidies commonly identify large amounts of subsidization as optimal policies (Basso & Silva, 2014) (Parry & Small, Should Urban Transit Subsidies Be Reduced?, 2009).

However, the effectiveness of this second-best strategy depends on the elasticity of substitution between private and public modes. Although transportation elasticities have been extensively investigated by economists, engineers and planners,[64] rare are the occasions where completely exogenous price chocks are observed, particularly in the case of non-marginal changes as the ones corresponding to the large subsidizations recommended by the structural models on optimal fare pricing. Moreover, it has been observed that reduced form estimations of elasticities tend to be lower if compared to typical structural models (Kremers, Nijkamp, & Rietveld, 2002),

[62] See for example, http://goo.gl/yhGZU7.

[63] Transit subsidies can also be theoretically supported based on scale economies relating to fixed costs and the "Mohring effect" (Parry & Small, 2009) and economies of density (Elgar & Kennedy, 2005)

[64] Some literature reviews and meta analyses on public transit elasticities include (Litman, 2017), (Kremers, Nijkamp, & Rietveld, 2002), (Holmgren, 2007). Moreover (Volinski, 2012) reviews the outcomes of fare-free experiments in the USA

which might be problematic given the high sensitivity of optimal pricing policies to these parameters.

Within this context, my study aims to explore as a quasi-experiment the Brazilian policy that grants fare free public transportation to older adults. By comparing the travel behavior of individuals who are just below and just above the policy threshold I can infer through a regression discontinuity (RD) design the causal effect of a total fare subsidization, and thus compare my results with traditional elasticity parameters from the literature.

In recent years, RD designs have become a popular method in applied economics given its theoretically capacity of recovering causal effect given considerably mild assumptions. In a meta-analysis evaluating the method effectiveness, RD designs have shown not only high performance in terms of internal validity but also for the external validity of its results (Chaplin, et al., 2018). To the extent of my knowledge, no other study has yet explored this method to identify the causal effects of price changes on travel behavior.

3.1.2 Reduced fare Programs for the Elderly

Reduced fare programs for the elderly are a common practice worldwide. National policies include the UK concessionary fares and the Brazilian fare-free public transit fare for all individuals older than 65. However, while there is a large literature evaluating the outcomes of the UK policy, not many academic papers have been published investigating the Brazilian case.

Possibly the only exception is Pereira et al. (2015), which estimates the impact of population aging on the share of transit trips made by senior individuals eligible for fare exemption in São Paulo. Using demographic projection methods, the authors estimate that the share of transit trips made by senior individuals will increase by 126.7% by 2050. So, according to the authors, in order to maintain the current level of subsidization, the transit fare for paying passengers should increase by 21% if the current fare-free scheme to the elderly is not changed.

The Brazilian Constitution mandates that urban public transportation should be free of charge for all individuals above the age of 65.[65] This benefit is valid throughout the country and cannot be overruled by local regulations. Moreover, in 1993, the city of São Paulo extended the benefit by reducing to 60 the age threshold for fare-free eligibility for women. In 2013, a similar threshold reduction was enacted for the male population.[66] Therefore, between 1993 and 2013, while women could start riding public transit for free at age 60, men had the same benefit only when they turned 65. It is important to notice that this lower age threshold for women in São Paulo was only valid for trips made by municipal buses, which account for 57.4% of all public transit trips in the city (METRO, 2013). In the case of trips made by other public modes such as subway, urban trains and metropolitans buses, the fare exemption age threshold was 65 for both men and women.[67]

So how does this fare exemption to the elderly work in practice? For having the right to board without paying, eligible elders are only required to present an official document displaying their date of birth. This ID can be shown either to the bus driver, ticket collector or subway personnel in the entrance of each station. Additionally, eligible seniors have the option to apply for a special transit card, which can be used on automated ticketing equipment of buses and transit stations, therefore avoiding the necessity of displaying a valid ID when boarding a bus or a train.

An important concern when estimating the fare-free policy effects on ridership is the possibility of frauds. For example, people who are not eligible for the benefit could be using fake IDs or senior transit cards from eligible elders. If too many individuals do not abide by the rules of the regulation, then any policy effects would be biased towards zero. Unfortunately, the exact number of people committing fraud is not easily identifiable. In 2015, the Transit Agency of São Paulo implemented a system of facial recognition in the ticketing machines of some buses, and in that year, 3,337 people were caught using senior transit cards from other people. However, when compared to the 1.09 million senior transit cards, the number of detected frauds corresponded to a

[65] https://www.senado.gov.br/atividade/const/con1988/CON1988_05.10.1988/art_230_.asp

[66] Municipal Law nº 11.381 of June 17, 1993 and Municipal Law nº 15.912 of December 16, 2013.

[67] In 2013, the state government also reduced the age threshold for fare exemption in those modes to 60 years old (State Law nº 15.187, of October 29, 2013). However, the analysis carried in this paper is restricted to the years of 2007 and 2012, therefore it is not affected by this policy change.

small fraction of the total.[68] Therefore, unless the total number of frauds is much higher than the number of individuals currently being detected committing such crimes, this specific issue is unlikely to severely bias my main estimates.

Besides that, are eligible individuals aware of their benefit? Another concern that could also distort the estimation of policy effects towards zero is the awareness of beneficiaries about the fare-free benefit. Eligible elders who are not aware of their fare exemption would not change their travel behavior. However, in a survey conducted with elders from all over the country, 94% of respondents reported being aware of the benefit to ride public transportation for free (Néri, 2007). Specific for the case of São Paulo, Martins & Massarollo (2010)also indicate that the eligibility for fare-free public transportation was the most well know right of the elderly among a sample of senior individuals.

Still, although most people are aware of the benefit, about a quarter of eligible elderly report paying for their trips (METRO, 2013). Therefore, all estimates of policy effects in this paper should be interpreted as intention-to-treat effects, i.e., the effects of the policy conditional on the willingness of individuals to exercise their benefit.

Finally, is the number of people who ride for free relevant? According to a report from the São Paulo Secretary of Transportation, in 2017, 10% of total bus ridership in the city is composed by trips made by fare-exempted elders. This share corresponds to approximately 14.8 million trips per month.[69] This same report indicates that the exemption to the elderly costs about R$ 1 billion[70] per year, or 1.85% of the overall city government budget.[71]

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[68] The number of repealed transit cards was based on the information provided by the São Paulo Transit Agency due to Request 12,472 to the Law of Access to Public Information. The total number of activated elderly cards is based on Request 14,536.

[69] http://www.prefeitura.sp.gov.br/cidade/secretarias/upload/transportes/SPTrans/acesso_a_informacao/2017/detalhamento-planilha-tarifaria-reajustejan17.xlsx.

[70] Equivaled to approximately U$320 million.

[71] http://www.camara.sp.gov.br/transparencia/orcamentos-da-camara/orcamento-2017/

3.2 Data

My source of information about individuals' travel behavior are a set of household travel surveys from two of the largest Brazilian Metropolitan Areas, namely São Paulo and Belo Horizonte.[72] These surveys were designed to be representative of all trips made by the population of each metro area in a regular working day. For the case of São Paulo, the surveys were carried in 2007 and 2012. For Belo Horizonte, the surveys are from 2002 and 2012. In each of these datasets, a sample of households was randomly selected, and individuals were asked to report all trips taken in the day immediately before the interview.[73] The information about trips include: motivation, transport modes, time of departure and arrival, and geocoded origins and destinations. Additionally, the surveys also include a rich compilation of respondent's socioeconomic characteristics such as age, sex, education attainment, income, employment and vehicle ownership.

Table 2.1 shows the basic descriptive statistics of observations from these surveys.[74] They include 336,217 individuals, out of which 43,340 were between 55 to 69 years old. I highlight older age adults because these are the relevant observations for my empirical estimation. About a third of the sample had completed secondary education and about half of it owned a private vehicle. Both these statistics are consistent among individuals who were closer to the exemption thresholds. However, not surprisingly, the older were the individuals, the larger was the portion of retirees. Additionally, while 63% of the total sample was observed making at least one trip, this share was decreasing with age.

The relationship between age and travel behavior is important because a key underlying assumption for the RD estimation is the continuity in the association between the running and the dependent variables. In this case, the running variable is the age of individuals, and the dependent variables are the number of trips made by different modes. Therefore, in order to illustrate the relationship between these variables, Figure 3.1 shows the average number of trips by age in the sample. From the figure, there does not seem to exist any major discontinuities between the two

72 São Paulo is Brazilian largest metropolitan Area, with 21 million residents, and Belo Horizonte is Brazilian 3rd largest metro area, with 5.8 million residents (IBGE, 2016).

73 Interviews were carried from Tuesdays to Saturdays, so all information refers to trips made during regular weekdays.

74 Appendix A presents additional descriptive statistics of individuals in our sample, including a comparison of surveys used in the analysis.

variables. The average number of trips grows rapidly from early ages up to the end of high-school years. After reaching a lifetime max at age 18, the average number of trips decreases to about 0.8 trips per person around the age of 20, and remains mostly flat until the age of 40. From that age and beyond, the average number of trips decreases almost linearly.[75]

Table 3.1: Household Survey – Descriptive Characteristics of Individuals

	all data		by age					
			55-59		60-64		65-69	
	obs.	share[a]	obs.	share[a]	obs.	share[a]	obs.	share[a]
Total Individuals	336,217	100.0%	17,032	100.0%	14,504	100.0%	11,804	100.0%
Sex								
male	159,091	47.3%	7,664	45.0%	6,386	44.0%	5,149	43.6%
female	177,126	52.7%	9,368	55.0%	8,118	56.0%	6,655	56.4%
Work Status								
working	141,140	42.0%	7,824	45.9%	4,583	31.6%	2,045	17.3%
retired	42,815	12.7%	4,418	25.9%	6,402	44.1%	7,653	64.8%
Education								
high-school	122,185	36.3%	6,449	37.9%	5,085	35.1%	3,589	30.4%
Vehicles								
owns car	172,952	51.4%	10,017	58.8%	8,351	57.6%	6,376	54.0%
Trips								
travels	212,967	63.3%	9,079	53.3%	6,896	47.5%	4,893	41.5%
by car	60,638	18.0%	3,662	21.5%	2,898	20.0%	1,909	16.2%
by pub. transit	64,724	19.3%	2,914	17.1%	2,192	15.1%	1,776	15.0%
by walking	78,429	23.3%	2,594	15.2%	1,925	13.3%	1,323	11.2%
by bus	54,308	16.2%	2,411	14.2%	1,870	12.9%	1,507	12.8%
by rail	10,677	3.2%	510	3.0%	332	2.3%	273	2.3%
driving	36,479	10.8%	2,746	16.1%	2,100	14.5%	1,350	11.4%
by car ride	18,371	5.5%	750	4.4%	677	4.7%	480	4.1%
to work	110,215	32.8%	5,694	33.4%	3,387	23.4%	1,681	14.2%
to other reasons	112,445	33.4%	5,090	29.9%	3,770	26.0%	2,506	21.2%
Survey								
Belo Horizonte 2002	121,296	36.1%	4,810	28.2%	4,156	28.7%	3,457	29.3%
Belo Horizonte 2012	100,656	29.9%	5,840	34.3%	4,882	33.7%	3,978	33.7%
São Paulo 2007	89,970	26.8%	5,019	29.5%	4,258	29.4%	3,488	29.5%
São Paulo 2012	24,295	7.2%	1,363	8.0%	1,208	8.3%	881	7.5%

Notes: [a] shares are based on the total for each column.

[75] Similar plots for other dependent variables used in the empirical analysis are presented in Appendix B.

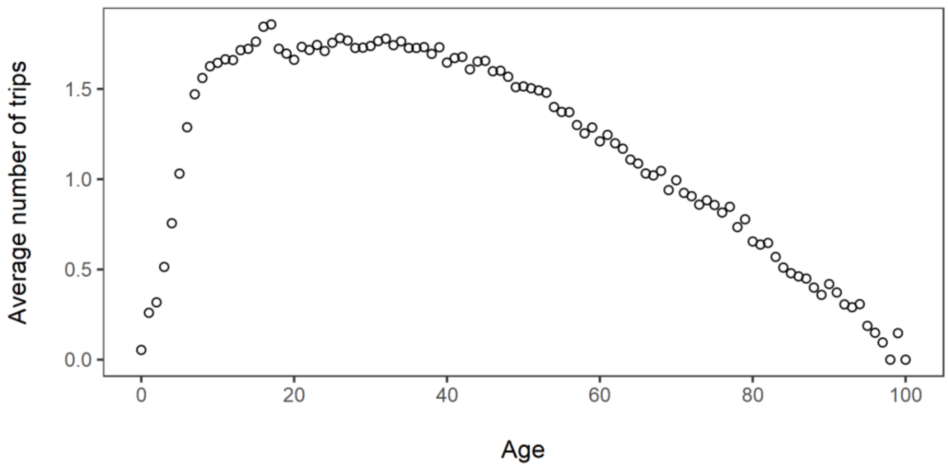

Figure 3.1: Average number of Trips by Age

3.3 Empirical Strategy and Results

In this session, I carry my empirical analysis used to identify the effects of the transit fare exemption policy on the travel behavior of its beneficiaries. I start by investigating the adherence of individuals to the fare-free benefit. Then, I describe the empirical strategy used for the identification of the treatment effect, detailing the main underlying assumptions for the validity of results. Next, I estimate the average treatment effect using a pooled sample that includes all individuals in my sample. Thereafter, I explore the different age eligibility thresholds for women in Belo Horizonte and São Paulo to investigate potentially confounding unobserved discontinuities at the policy cutoff ages. Finally, I try to identify heterogeneities on the treatment effects by running separated estimations for different segments of the population.

3.3.1 Adherence to the Benefit

My first analysis investigates the adherence of eligible individuals to the policy benefits. I explore the fact that in the household travel surveys of 2007 from São Paulo, and 2012 from Belo Horizonte, people who reported traveling by public transportation were also asked to indicate who paid for the trips. Among the alternatives for answering this question, respondents could indicate if they had traveled for free. Therefore, on Figure 3.2, I plot the share of transit travelers who reported riding for free by age-distance to their corresponding exemption threshold.[76]

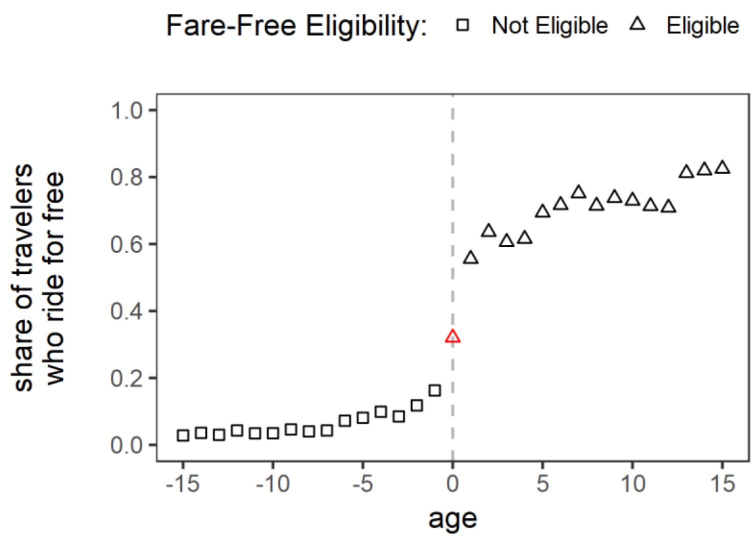

Figure 3.2: Share of Transit Travelers Who Report Riding For Free By Age

In theory, all individuals could start riding public transit for free at the same date when they reach the policy threshold age. However, as it is clear on Figure 3.2, those who are exactly at the cutoff age are not as likely to take advantage of the benefit as those who are at least one year older. A few reasons could explain this fact. First, it may take some time for individuals to become aware of the benefit, so those who are just at the age threshold may still not know about their right to ride for free. Additionally, most of the fare exempted trips are made with the use of senior transit cards (Prefeitura de São Paulo, 2017). However, for getting access to these cards, eligible elders must fill an application with the transit agency of their city. This process requires the submission of documents attesting the fulfillment of policy eligibility requirements.[77] Although the application can be started as early as three months before their eligibility birthday, some elders may delay this process, therefore limiting their ability to fully benefit from the fare exemption policy in the first

[76] Women from São Paulo were subject to a different eligibility threshold age (60) if compared to the other groups in the sample (65).
[77] http://bilheteunico.sptrans.com.br/especial.aspx

few months of their eligibility period. Hence, the treatment status of individuals who are exactly at the threshold age may be ambiguous. So, to avoid any bias due to misidentification of treatment status, I exclude these observations from the baseline estimation of the RD model.[78]

Nevertheless, there seems to be a clear effect of eligibility for senior's free riding on the share of travelers who report riding for free, which goes from less than 20% in the group of individuals below it, to more than 60% for those above the policy threshold age. However, it is important to notice two additional aspects revealed by this figure, 1) there are individuals who ride for free but who are younger than the threshold age; 2) there are people above the threshold age but who pay for their trips.

The first case can be explained by the fact that other fare exemption rules exist in both São Paulo and Belo Horizonte. Besides the elderly, public workers such as policemen, firefighters and mailmen can ride public transportation for free regardless of their age.[79] Moreover, physically handicapped individuals can also apply for the benefit, which may explain the increasing share of free-riders before the senior's exemption threshold age.

As for the elders eligible for fare exemption who pay for the transit fare, a lack of awareness does not seem to explain that behavior, as 93% of elders are aware of the benefit (Néri, 2007). An alternative explanation for the existence of this group could be that disadvantaged individuals could face some difficulty in exercising their right to ride for free, as for example, not having a valid ID to prove their age. To investigate this hypothesis, I run a balance test of observed characteristics comparing eligible individuals (older than 65) who pay for the fare with those in the same age group but who report riding for free. The results of this comparison are shown in Table 3.2. On average, the individuals who pay for the transit fare are wealthier, more educated,

[78] The exclusion of observations close to the running variable threshold is referred in the literature as a "Donut-RD". This procedure is most common in cases of heaping data at the running variable threshold (Almond & Jr, 2011), (Eggers, Fowler, Hainmueller, Hall, & Jr., 2015). Although it is undesirable to exclude this data points, the first alternative would be to consider them as treated, which would likely misidentify several individuals who may not be able to ride for free, thus biasing any discontinuity estimate towards zero. Alternatively, I could try to identify in the data those individuals who reported traveling for free and those who did not, and then use this information to assign treatment status. The main issues associated with this procedure are twofold; first it is unclear how to assign treatment status for individuals who do not travel by public transportation at all, in which case we cannot be sure if they would or would not pay for the fare. Secondly, even if identification of treatment status was perfect, that would lead to the issue of selection into treatment, as those exactly at the threshold who were more likely to desire free trips would also be more likely to register earlier for senior transit cards.

[79] http://www.bhtrans.pbh.gov.br/portal/page/portal/portalpublico/Temas/Onibus/gratuidade-2013 and http://bilheteunico.sptrans.com.br/especial.aspx

more likely to own a vehicle and more likely to be employed[80] than the group of individuals who report traveling for free. Therefore, lower access to the policy by disadvantaged individuals may not be the main factor explaining why some people pay for the transit fare even when they are eligible to ride for free. Instead possible explanations include personal pride or irrelevance of the cost for wealthier individuals.

Given these results it is important to highlight that further RD results can be interpreted as intention to treat effects since the treatment variable that is being analyzed is the eligibility for free-riding, and not the act of traveling by public transportation for free.

Table 3.2: Balance Test Between Payers and Free-Riders Transit Riders Eligible for Fare Exemption (Age > 65)

Variable	free riders	fare payers	t.stat	p.value
female	0.562	0.583	-0.871	0.384
age	73.2	72.7	1.880	0.060
works	0.114	0.168	-3.042	0.002 **
retired	0.807	0.763	2.176	0.030 *
high school	0.294	0.344	-2.193	0.029 *
owns car	0.386	0.412	-1.066	0.287
income	2,322	2,873	-2.847	0.005 **

Notes: *** $p<0.001$, ** $p<0.01$, * $p<0.05$,

3.3.2 Pooled RD Estimation

To identify how the policy of fare-free public transportation affects the travel behavior of individuals, I employ a regression discontinuity design (RDD). In synthesis, this method compares the average number of trips made by individuals who are just younger and just older than the policy eligibility threshold. The main advantage of this empirical approach is that the underlying assumptions needed for the internal validity of results are relatively weak. Basically, it is assumed that the unobserved characteristics that affect travel behavior are similar for individuals on both sides of the policy threshold, and that the relationship between the running variable (age), and

[80] The comparison already excludes individuals who report that the transit fare was paid by the employer.

dependent variable (number of trips) should be described by a continuous function. If these assumptions hold, then any discontinuities in the dependent variable at the threshold age could only be attributed to the policy effect.

Because different groups of people become eligible for fare exemption at different ages, I normalize the running variable d_i as the difference of individuals' age to their eligibility threshold:

$$d_i = age_i - age_i^o \tag{1}$$

Where age_i is the age of individual i, age_i^o is the age threshold for fare exemption eligibility for that particular individual, and d_i is the difference between those values. From the normalized running variable, the treatment status T_i of individuals in my sample is defined as:

$$\begin{aligned} T_i = 0 \qquad if \qquad d_i < 0 \\ T_i = 1 \qquad if \qquad d_i \geq 0 \end{aligned} \tag{2}$$

Moreover, using this normalized running variable, I show on Figure 3.3 the graphical evidence about the relationship between the running and dependent variables of my study. Each panel of the figure show the average number of trips by age-distance to the policy threshold for different modes. In all four panels, we can verify a decreasing number of trips with age. As for discontinuities at the cutoff, Panel B seems to indicate that number of trips by public transportation goes up, and Panel D indicates a smaller discontinuity in the opposite direction for walking trips. As for the other modes, the graphical evidence is not clear, and no apparent trend discontinuity can be observed. To formally estimate the size and significance of these effects, I estimate a standard RD model, which can be described by the following equation:

$$y_{im} = \beta_{0m} + \beta_{1m} f_m(d_i) T_i + \beta_{2m} f_m(d_i) (1 - T_i) + \beta_{3m} T_i + \gamma_m X + \varepsilon_{im} \tag{3}$$

Where the outcome variable y_{im} is the number of trips made by individual i using mode m. Moreover, $f_m(d_i)$ is a continuous function that describes the relationship between the running variable d_i and the dependent variable. Furthermore, X_i is a vector of controls that include gender, city of residence, income, vehicle ownership and employment status. Finally, ε_{im} is a noise term that captures all other unobservables.

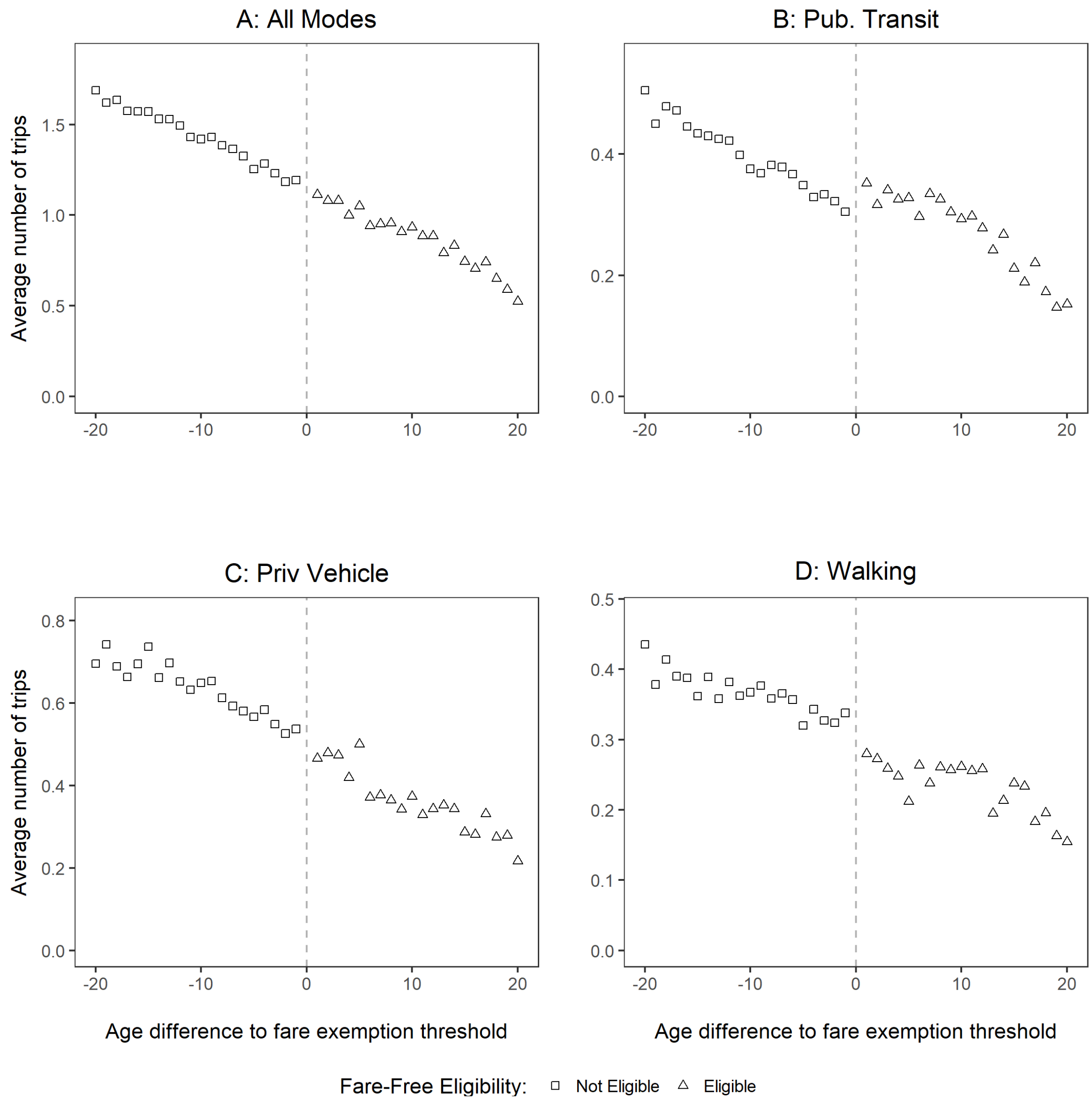

Figure 3.3: Average Number of Trips by Mode and by Distance to Fare Exemption Threshold

The main coefficient of interest is β_{3m}, which describes, for each mode, any discontinuity in the outcome variable at the policy age cutoff. Under the assumption of continuity of y_{im} on d_i at $d_i = 0$, β_3 is equivalent to the policy average treatment effect at the threshold (Imbens & Lemieux, 2007). In my baseline RD estimation, I use a linear approximation of $f_m(d_i)$ and I subset the estimation to a neighborhood of observations around the threshold using the non-parametrical minimum square error optimization procedure suggested by Calonico, Cattaneo, & Titiunik (2014). Observations within the selected bandwidth are weighted using a triangular kernel. Finally, standard errors are clustered by age, gender and metro area.

Using these baseline specifications,[81] Table 3.3 shows the results for the estimation of β_3 for different travel modes. The table presents first the absolute discontinuity in the average number of trips, and then the relative corresponding value given the counterfactual number of trips in the absence of the policy.[82]

In line with the graphical evidence presented in Figure 3.3, the main impact of the policy is an increase in the demand for public modes. The point estimate for public transit indicates that the fare-free policy increases the average number of trips of beneficiaries in 0.051 trips per capita, which corresponds to an increase of 22.8% in ridership among beneficiaries. As for the other modes, results are not statistically significant, and the largest coefficient in terms of magnitude is a decrease in approximately 9.6% in trips made by walking, although that results is not sufficiently precise for a final inference. The point estimate for private vehicle is particularly small, and if anything, positive, which indicates a relatively precise null effect of the policy on the demand for private modes.

Summing up, while there seems to be a positive effect of the policy on the demand for public transportation, the results do not indicate any significant effect associated with mode substitution.

[81] Appendix C Shows the consistent of these estimates to alternative RDD specifications

[82] Appendix D illustrates the interpretation of each of these coefficients.

Table 3.3: Fare Exemption Effect on the Average Number of Trips by Mode

	Trips by Travel Mode			
	all modes	pub. transit	priv. vehicle	walking
Fare-free Effect[1]				
Absolute	0.051	0.068 **	0.008	-0.031
	(0.036)	(0.021)	(0.028)	(0.022)
Relative	0.044	0.228 **	0.015	-0.096
	(0.032)	(0.071)	(0.053)	(0.068)
Covariates[2]	Yes	Yes	Yes	Yes
Bandwidth (yrs.)	8	8	8	8
Polin. Fit Degree	1	1	1	1
Kernel	Triang.	Triang.	Triang.	Triang.
Observations	30,069	30,069	30,069	30,069

Notes: º p<0.1; * p < 0.05; ** p < 0.01; *** p < 0.001

[1] Absolute effect refers to the chages in the average number of trips. Relative effects is the coefficient between the absolute effect and the counterfactual number of trips in the absence of the policy at the cutoff value, for example, a coefficient of 0.5 indicates an increase of 50% on

[2] Covariates include working status, car ownership, metropolitan area, income and gender.

3.4 Testing for Unobserved Disruptions at the Policy Threshold

The main threat to the validity of previous estimates is the existence of unobserved chocks that could affect travel behavior and be correlated with the age threshold used for defining the eligibility to the policy. This is particularly concerning in my empirical setup because the age cutoffs used for fare exemption eligibility are equivalent to the cutoffs used for other policies and befits for the elderly. Some examples include the minimum age for start collecting social security benefits in Brazil (60 years for women, 65 for men),[83] preferential lines on public offices, banks and other services (60 years),[84] preferred parking slots (60 years),[85] and reserved seats on public

[83] http://www.previdencia.gov.br/servicos-ao-cidadao/todos-os-servicos/aposentadoria-por-idade/

[84] http://www.planalto.gov.br/ccivil_03/leis/2003/L10.741.htm

[85] http://www.prefeitura.sp.gov.br/cidade/secretarias/transportes/autorizacoes_especiais/index.php?p=21225

transportation (60 years).[86] If the eligibility for these other benefits were to affect the demand for public transportation and other modes, then the RD estimates would not be capturing the causal effect of the fare exemption policy, but instead, the combined effect of all these policies.

To start investigating this question, I verify the existence of age trend discontinuities on observed variables that are unlikely to be caused by the fare exemption policy. To test the existence of these discontinuities, I estimate a similar model as the one employed in the previous session, however, instead of using the number of trips as the dependent variable, I use observed characteristics of individuals such as working status, vehicle ownership, education, place of residence and household income. Table 3.4 shows the resulting estimates of these regressions.[87]

Table 3.4: Discontinuities of Observed Individual Characteristics

	Dependent Var Dependent Variables: Changes in Individual Characteristics[1]					
	Retired	Employed	Owns Auto	Finished High-School	Household Income	Leaves in Capital City
Fare-free threshold						
Absolute Effect	0.065 ***	-0.035 **	-0.007	-0.011	-40.071	-0.017
	(0.012)	(0.011)	(0.012)	(0.012)	(166.334)	(0.012)
Relative Effect	0.133 ***	-0.126 **	-0.012	-0.033	-0.010	-0.027
	(0.025)	(0.040)	(0.022)	(0.035)	(0.040)	(0.019)
Covariates	No	No	No	No	No	No
Bandwidth (yrs.)	8	8	8	8	8	8
Polin. Fit Degree	1	1	1	1	1	1
Kernel	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.
Observations	37,156	37,156	36,906	37,156	15,138	37,156

Notes: º p<0.1; * p < 0.05; ** p < 0.01; *** p < 0.001

[1] Absolute effect refers to the chages in the average number of trips. Relative effects is the coefficient between the absolute effect and the counterfactual number of trips in the absence of the policy at the cutoff value, for example, a coefficient of 0.5 indicates an increase of 50% on average.

[86] http://www.planalto.gov.br/ccivil_03/leis/L10048.htm

[87] Appendix E includes a series of plots showing the association between each of these variables and the running variable of the RD model.

From this exercise, we can identify significant discontinuities associated with employment and retirement status of individuals. This result is not surprising because the Brazilian minimum age for workers to start collecting social security benefits coincides with the fare exemption threshold for a large portion of my sample.[88] For all other variables included in the analysis, the discontinuities are negligible. Anyway, the existence of a discontinuity in the likelihood of retirement, that coincides with the fare exemption age threshold triggers a warning for the internal validity of my main RD estimates. Although I include retirement and employment status as controls in the baseline regressions, if the effects of these concomitant chocks are not correctly captured by my covariates specification, that could lead to bias on my results.

To overcome this threat, I explore the earlier fare exemption eligibility that existed for women in São Paulo. For this analysis, I run two distinct sets of RD regressions, first using the subset of observations of women from São Paulo, and second, I restrict the sample to women from Belo Horizonte. In both cases, I use the age of 60 as the threshold for the RD estimation. Therefore, in the first set of regressions which use data of women from São Paulo, the model estimates the true fare exemption effect for that subgroup. However, in the case of the second set of regressions, which use data of women from Belo Horizonte, the estimations are placebo effects since the fare exemption did not start at age 60 in that city. However, if significant travel behavior changes were to occur at that age, and those were confounding my main results, then these effects should be observed in the placebo group.

Moreover, I also explore an additional aspect of the fare exemption threshold for women in São Paulo to test potentially confounding discontinuities. The eligibility for free public transportation at age 60 for women in São Paulo was limited to municipal buses. Therefore, for that group, I can also estimate a placebo model investigating confounding factors affecting the demand of women for public modes other than bus[89] at the threshold age of 60. Because women had no right for riding for free on trains at age 60, any discontinuity in ridership at that age could only be attributed to other unobserved chocks at the threshold.

[88] The threshold is the same for women from São Paulo (60 years), and men from São Paulo and Belo Horizonte (65 years).

[89] That basically corresponds to rail.

Table 3.5: Travel Behavior Discontinuities at the Age of 60 – Subsample of Female

	Dependent Variable: Changes in Trips by Travel Mode					
	all modes	pub. transit	priv. vehicle	walking	bus	rail (placebo effect)
São Paulo Women						
Absolute Effect	0.132	0.118 *	0.096	-0.081	0.118 *	0.000
	(0.091)	(0.056)	(0.071)	(0.056)	(0.050)	(0.028)
Relative Effect	0.095	0.302 *	0.163	-0.207	0.388 *	0.002
	(0.066)	(0.142)	(0.120)	(0.144)	(0.164)	(0.324)
Belo Horizonte Women						
Absolute Placebo Effect	-0.063	-0.039	0.011	-0.025		
	(0.048)	(0.029)	(0.031)	(0.033)		
Relative Placebo Effect	-0.069	-0.126	0.045	-0.072		
	(0.052)	(0.094)	(0.123)	(0.098)		
Covariates[2]	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth (yrs.)	8	8	8	8	8	8
Polin. Fit Degree	1	1	1	1	1	1
Kernel	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.

Notes: º p<0.1; * p < 0.05; ** p < 0.01; *** p < 0.001

[1] Absolute effect refers to the chages in the average number of trips. Relative effects is the coefficient between the absolute effect and the counterfactual number of trips in the absence of the policy at the cutoff value, for example, a coefficient of 0.5 indicates an increase of 50% on average.

[2] Covariates include working status, car ownership, metropolitan area, income and gender.

The results from these two regressions are presented on Table 3.5.[90] In the case of women from São Paulo, results mostly mirror what was observed in the pooled estimations. I find a significant increase in the share of trips by public transportation, a large, but non-significant reduction of walking trips and no significant effects in the case of trips by private vehicle or in the total number of trips. Supporting the internal validity of main results, the results associated with transit sub-modes in São Paulo show that all the ridership effect on public transit ridership for women was concentrated on buses, where they actually had the free-ride right. Meanwhile, on rail, where women still had to pay, the changes in ridership were found to be exactly zero.

[90] Appendix F shows the plots with the association between the dependent variables and the age of individuals included in each subgroup of this analysis

In the case of the placebo tests for women from Belo Horizonte, there is also no evidence of significant confounding effects at age. If anything, the results indicate a reduction in the use of public transportation at that age threshold, although the point estimate is not sufficiently precise.

All these results indicate that we cannot reject the assumption that are no unobserved chocks on travel demand associated with the fare eligibility threshold age, at least in the case of women from São Paulo. Therefore, we cannot reject the interpretation of our main results as the causal effect of the fare exemption policy.

3.5 Treatment Effect Heterogeneity

Finally, I investigate the heterogeneity of treatment effects on different groups of individuals. I run a series of RD estimations using different subsets of the population. Ideally, I would like to estimate the average treatment effect for all relevant combinations of individual characteristics. However, one of the main limitations of the RDD is that estimations are restricted to the neighborhood of the running variable threshold, limiting the statistical power of estimations (Angrist & Pischke, 2008), therefore meaningful inference for small subsets of the population is not feasible with my sample.

Given this limitation, I estimate a series of models using different partitions of my total sample. First, I compare the results for subsets defined in terms of gender and metro area. I then investigate how individuals of different socioeconomic backgrounds are differently affected by the fare exemption, splitting the sample in terms of income, education, vehicle ownership and place of residence. Results from all these estimations are presented on Table 3.6. For concision, the table only includes the relative effects of the policy. In general, results do not differ from the main estimates of the pooled model. The larger and most significant effects are positive chocks on the demand for public transportation, meanwhile, most coefficients associated with other modes were not significant.

Table 3.6: Treatment Effect Heterogeneity

	Dependent Variable: Changes in Trips by Travel Mode[1]			
	all modes	pub. transit	priv. vehicle	walking
Sex				
Male	0.058	0.237 *	0.038	-0.096
	(0.046)	(0.110)	(0.071)	(0.111)
Female	0.036	0.223 *	-0.004	-0.095
	(0.044)	(0.092)	(0.079)	(0.087)
Metro Area				
São Paulo	0.092 °	0.313 **	0.067	-0.087
	(0.051)	(0.115)	(0.082)	(0.124)
Belo Horizonte	0.012	0.178 °	-0.026	-0.108
	(0.044)	(0.095)	(0.078)	(0.083)
Car Ownership				
Owns Car	0.011	0.165	0.027	-0.159
	(0.040)	(0.108)	(0.058)	(0.099)
Doesn't Own Car	0.119 *	0.261 **	0.007	-0.039
	(0.055)	(0.090)	(0.207)	(0.092)
Education				
High School	-0.006	0.149	0.024	-0.216 *
	(0.042)	(0.111)	(0.064)	(0.097)
No High School	0.066	0.235 **	-0.019	-0.010
	(0.042)	(0.078)	(0.022)	(0.082)
Covariates[2]	Yes	Yes	Yes	Yes
Bandwidth (yrs.)	8	8	8	8
Polin. Fit Degree	1	1	1	1
Kernel	Triang.	Triang.	Triang.	Triang.

Notes: ° p<0.1; * p < 0.05; ** p

[1] All coefficients are presented as relative effects. That is, a coefficient of 0.5 indicates an increase of 50%.

[2] Covariates include working status, car ownership, metropolitan area, income and gender.

As for the heterogeneities of estimates, effects do not present any significant differences by gender, with roughly the same point estimates men and women. In the case of Metro Areas, the effect on ridership was found to be larger in São Paulo (31.3%) if compared to Belo Horizonte (17.8%). Moreover, in the case of São Paulo the coefficient associated with the total number of trips was slightly significant, indicating that the policy effect on transit demand was sufficiently large to become visible in the overall number of trips in the city.

The remaining comparisons are related to variables that are associated with socioeconomic status, and their results are well aligned. For both the individuals who do not own a car and have not completed secondary education, the effects of free-fare on transit ridership were larger, which is not surprising since the use of public transportation is concentrated among less wealthy individuals. In the case of people who owned a car or have completed high-school, point estimates were still positive, but no longer significant. This result is particularly relevant for starting to understand the mechanisms behind the null results of the policy on mode substitution. The response of wealthier individuals who own private vehicles do not seem to be a substitution between private to public modes. Instead, for this group what we observe is a reduction in walking trips associated with the policy. This result could be associated with the better transit infrastructure and the higher number of urban opportunities in wealthier parts of the city. Within these regions, it seems likely that one could substitute a walking trip with a short transit ride. However, in the case of suburb residents, not many opportunities are available at walking distance, so the additional transit ridership caused by the fare exemption is more likely to be associated with longer routes that otherwise would not be made.

Summing up, while the policy seems to have a larger direct impact on the demand for public transportation for the more disadvantaged, the only effect for wealthier individuals seems to be a substitution from walking, a result that is the opposite of the expected if the policy had the goal to reduce traffic related externalities.

3.6 Conclusion

This paper has investigated how a policy of free-fare public transportation affects the travel behavior of beneficiaries. I have first shown that, in general individuals who have the right to travel for free do take advantage of this benefit when riding public transportation. Next, I have shown that the policy is associated with a significant and consistent increase in average transit ridership. I ruled out the hypothesis that this effect could be associated with unobserved chocks concomitant with the policy threshold. Finally, I've found that the ridership effect was mostly concentrated on individuals with lower socioeconomic characteristics and who do not own a car.

With respect to mode substitution, I have not found any evidence of a reduction in the use of private vehicles associated with the eligibility for free public transportation. If anything, we found preliminary evidence that wealthier individuals may actually be substituting walking trips for public transportation due to the fare exemption policy.

Contrary to the policy recommendations from (Basso & Silva, 2014) and (Parry & Small, 2009), our results suggest that subsidizing the public transit fare may be of limited effectiveness for reducing the social costs associated with the externalities of private vehicles use. Instead our results favor the analysis of (Storchmann, 2003), who argued that automobile externalities should not be answered by transit fare subsidies and that externalities would be better addressed by policies aimed to directly internalize external costs.

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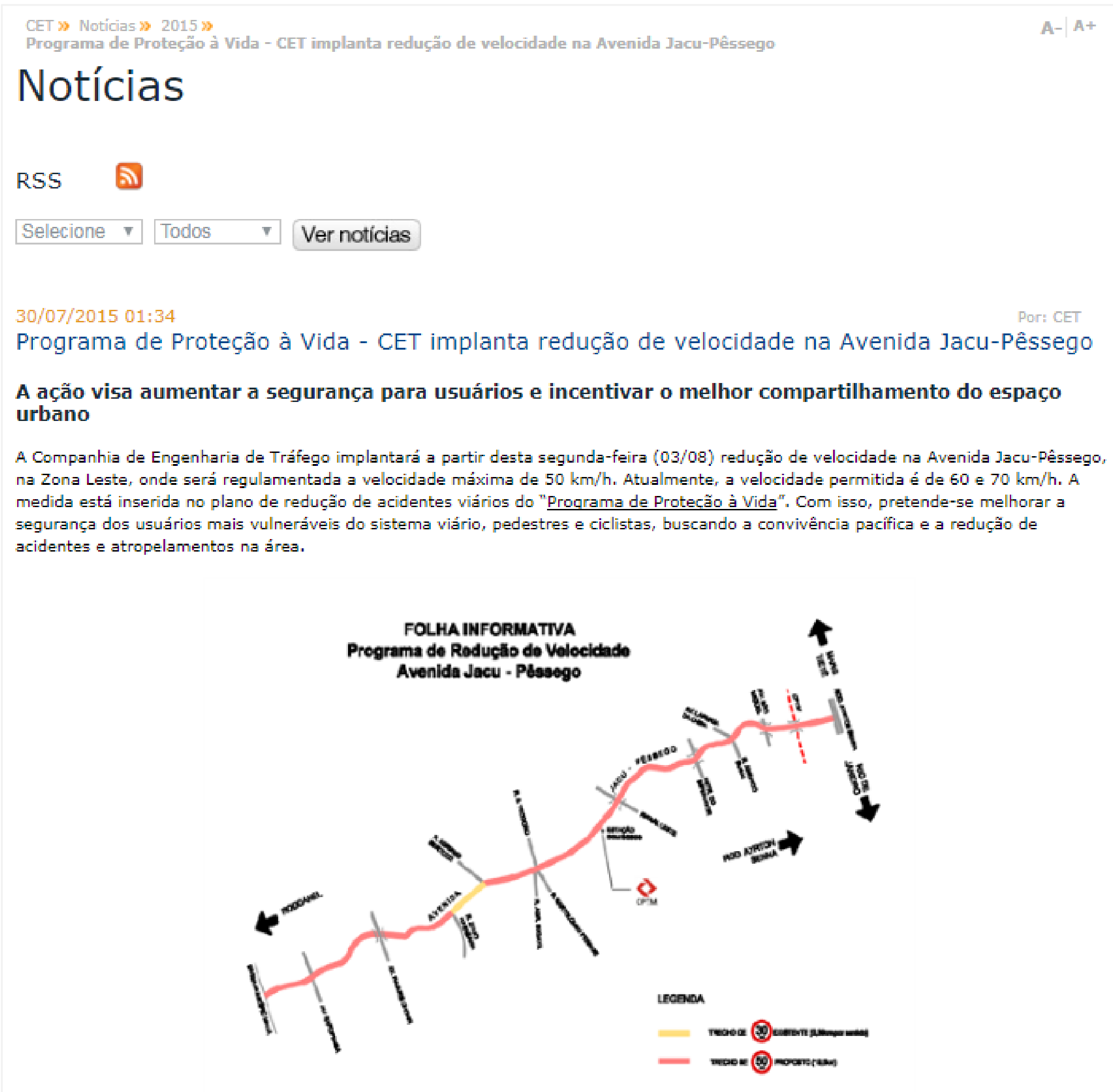
Appendix A: Chapter 1 Appendix



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Programa de Proteção à Vida - CET implanta redução de velocidade na Avenida Jacu-Pêssego

A ação visa aumentar a segurança para usuários e incentivar o melhor compartilhamento do espaço urbano

A Companhia de Engenharia de Tráfego implantará a partir desta segunda-feira (03/08) redução de velocidade na Avenida Jacu-Pêssego, na Zona Leste, onde será regulamentada a velocidade máxima de 50 km/h. Atualmente, a velocidade permitida é de 60 e 70 km/h. A medida está inserida no plano de redução de acidentes viários do "Programa de Proteção à Vida". Com isso, pretende-se melhorar a segurança dos usuários mais vulneráveis do sistema viário, pedestres e ciclistas, buscando a convivência pacífica e a redução de acidentes e atropelamentos na área.

Figure A.1: Traffic Agency Report from 2015 Indicating Details of an upcoming Speed Limit Reduction

Table A.1: 2015 Speed Limit Change Reports

Announcement Date	Speed Limit Change Date	Treated Roads	Link
7/8/2015	7/20/2015	AV MARGINAL DO RIO TIETE, AV MARGINAL DO RIO TIETE	http://www.capital.sp.gov.br/noticia/velocidade-maxima-das-marginais-sera-reduzida-a
7/30/2015	8/3/2015	AV JACU-PESSEGO, AV JACU-PESSEGO	http://www.cetsp.com.br/noticias/2015/07/30/programa-de-protecao-a-vida-cet-implanta-reducao-de-velocidade-na-avenida-jacu-pessego.aspx
7/30/2015	8/3/2015	AV ARICANDUVA, VD ENG ALBERTO BADRA	http://www.cetsp.com.br/noticias/2015/07/30/programa-de-protecao-a-vida-cet-implanta-reducao-de-velocidade-na-avenida-aricanduva.aspx
7/30/2015	8/3/2015	AV S JOAO, AV GAL OLIMPIO DA SILVEIRA, RUA AMARAL GURGEL	http://www.cetsp.com.br/noticias/2015/07/30/programa-de-protecao-a-vida-cet-implanta-reducao-de-velocidade-no-eixo-sao-joao-olimpio-da-silveira-amaral-gurgel.aspx
8/12/2015	8/17/2015	AV ANGELICA, AV ANGELICA, AV NADIR DIAS DE FIGUEIREDO, RUA MAJ NATANAEL, AV DR ABRAAO RIBEIRO, AV PACAEMBU	http://www.cetsp.com.br/noticias/2015/08/12/cet-implanta-cet-implanta-reducao-de-velocidade-maxima-em-mais-duas-vias.aspx
8/17/2015	8/20/2015	AV AFRANIO PEIXOTO, AV VALDEMAR FERREIRA, RUA HENRIQUE SCHAUMANN, AV PAULO VI, AV SUMARE, AV ANTARTICA, AV PROF MANUEL JOSE CHAVES, AV CARLOS CALDEIRA FILHO, AV VER JOSE DINIZ, ES DO CAMPO LIMPO	http://www.cetsp.com.br/noticias/2015/08/17/cet-implanta-reducao-de-velocidade-maxima-em-mais-11-vias-da-cidade.aspx
8/20/2015	8/23/2015	RUA DOMINGOS DE MORAIS, AV GUARAPIRANGA, ES M'BOI MIRIM, AV SEN TEOTONIO VILELA, AV ARNOLFO AZEVEDO, RUA ALM PEREIRA GUIMARAES, RUA DOMINGOS DE MORAIS	http://www.cetsp.com.br/noticias/2015/08/20/cet-implanta-reducao-de-velocidade-maxima-em-mais-6-vias-da-cidade-(1).aspx
8/24/2015	8/27/2015	AV PEDROSO DE MORAIS, AV PROF FONSECA RODRIGUES, AV DR GASTAO VIDIGAL	http://www.cetsp.com.br/noticias/2015/08/24/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias-na-cidade.aspx
8/27/2015	8/31/2015	PTE ENG ARY TORRES, AV DOS BANDEIRANTES, AV AFFONSO D'ESCRAGNOLLE TAUNAY, CV MARIA MALUF, AV SANTOS DUMONT, AV TIRADENTES, AV PRESTES MAIA, TN PAPA JOAO PAULO II, AV VINTE E TRES DE MAIO, AV RUBEM BERTA, AV MOREIRA GUIMARAES, AV WASHINGTON LUIS, AV INTERLAGOS, AV WASHINGTON LUIS	http://www.cetsp.com.br/noticias/2015/08/27/cet-implanta-reducao-de-velocidade-maxima-em-mais-16-vias.aspx
9/3/2015	9/9/2015	AV SALIM FARAH MALUF, AV JUNTAS PROVISORIAS, RUA MALVINA FERRARA SAMARONE, AV PRES TANCREDO NEVES	http://www.cetsp.com.br/noticias/2015/09/03/cet-implanta-reducao-de-velocidade-maxima-em-mais-4-vias.aspx
9/4/2015	9/11/2015	AV FRANCISCO MATARAZZO, VD LESTE-OESTE, AV ALCANTARA MACHADO, RUA MELO FREIRE, AV CD DE FRONTIN, AV ANTONIO ESTEVAO DE CARVALHO, RUA DR LUIZ AYRES, RUA ENG SIDNEY APARECIDO DE MORAES, AV JOSE PINHEIRO BORGES	http://www.cetsp.com.br/noticias/2015/09/04/cet-implanta-reducao-de-velocidade-maxima-no-eixo-leste-oeste.aspx
9/14/2015	9/18/2015	RUA CARMOPOLIS DE MINAS, AV BANDEIRANTES DO SUL, RUA CEL GUILHERME ROCHA, RUA CIRO SOARES DE ALMEIDA, AV OLAVO FONTOURA, AV EDUC PAULO FREIRE	http://www.cetsp.com.br/noticias/2015/09/14/cet-implanta-reducao-de-velocidade-maxima-em-mais-7-vias.aspx
9/18/2015	9/23/2015	AV PEDRO ALVARES CABRAL, AV BRASIL, AV JABAQUARA, AV JABAQUARA	http://www.cetsp.com.br/noticias/2015/09/18/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
9/22/2015	9/25/2015	AV DO ESTADO, AV DO ESTADO, AV ATLANTICA	http://www.cetsp.com.br/noticias/2015/09/22/cet-implanta-reducao-de-velocidade-maxima-em-mais-2-vias.aspx
9/24/2015	9/30/2015	AV VITOR MANZINI, PTE DO SOCORRO	http://www.cetsp.com.br/noticias/2015/09/24/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx
9/30/2015	10/2/2015	AV DOM PEDRO I, RUA TEREZA CRISTINA, AV NAZARE, AV DR RICARDO JAFET, AV DR RICARDO JAFET, AV PROF ABRAAO DE MORAIS	http://www.cetsp.com.br/noticias/2015/09/30/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
10/1/2015	10/7/2015	RUA MANUEL DA NOBREGA, AV REPUBLICA DO LIBANO, AV INDIANOPOLIS,	http://www.cetsp.com.br/noticias/2015/10/01/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx
10/6/2015	10/9/2015	AV BRIG FARIA LIMA, RUA DOS PINHEIROS, AV HELIO PELEGRINO, RUA INHAMBU, TN SEBASTIAO CAMARGO, AV PRES JUSCELINO KUBITSCHEK, CV TRIBUNAL DE JUSTICA, RUA ANTONIO MOURA ANDRADE, CV AYRTON SENNA	http://www.cetsp.com.br/noticias/2015/10/06/cet-implanta-reducao-de-velocidade-maxima-em-mais-9-vias.aspx
10/9/2015	10/14/2015	AV PRES WILSON, RUA S RAIMUNDO, RUA S RAIMUNDO, RUA MANOEL PEREIRA DA SILVA, RUA MANOEL PEREIRA DA SILVA, AV DR FRANCISCO MESQUITA	http://www.cetsp.com.br/noticias/2015/10/09/cet-implanta-reducao-de-velocidade-maxima-em-mais-4-vias.aspx
10/14/2015	10/16/2015	AV REBOUCAS, AV EUSEBIO MATOSO, TN JORN FERNANDO VIEIRA DE MELO	http://www.cetsp.com.br/noticias/2015/10/14/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx

Table A.1: 2015 Speed Limit Change Reports (Continuation)

Announcement Date	Speed Limit Change Date	Treated Roads	Link
10/16/2015	10/21/2015	AV PROF FRANCISCO MORATO, AV EMERICO RICHTER	http://www.cetsp.com.br/noticias/2015/10/16/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx
10/20/2015	10/23/2015	AV DR ARNALDO, AV JORN ROBERTO MARINHO, PTE OCTAVIO FRIAS DE OLIVEIRA, AV JOAO SIMAO DE CASTRO	http://www.cetsp.com.br/noticias/2015/10/20/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias.aspx
10/22/2015	10/28/2015	AV ROQUE PETRONI JUNIOR, AV PROF VICENTE RAO, AV VER JOAO DE LUCA, RUA JUAN DE LA CRUZ, AV CUPECE	http://www.cetsp.com.br/noticias/2015/10/22/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
10/26/2015	10/30/2015	AV DR HUGO BEOLCHI, AV ENG ARMANDO DE ARRUDA PEREIRA, AV ENG GEORGE CORBISIER	http://www.cetsp.com.br/noticias/2015/10/26/cet-implanta-reducao-de-velocidade-maxima-em-mais-3-vias-(1).aspx
10/29/2015	11/4/2015	AV CORIFEU DE AZEVEDO MARQUES, AV VITAL BRASIL, AV DOS TAJURAS, TN PRES JANIO QUADROS, AV LINEU DE PAULA MACHADO	http://www.cetsp.com.br/noticias/2015/10/29/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
11/4/2015	11/6/2015	PTE ENG ROBERTO ROSSI ZUCCOLO, AV CIDADE JARDIM, TN MAX FEFFER, AV EUROPA, RUA COLOMBIA, RUA AUGUSTA, RUA NOVE DE JULHO	http://www.cetsp.com.br/noticias/2015/11/04/cet-implanta-reducao-de-velocidade-maxima-em-mais-7-vias-(1).aspx
11/16/2015	11/19/2015	AV ELISEU DE ALMEIDA, RUA PIRAJUSSARA, AV INTERCONTINENTAL, AV JAGUARE, AV ESCOLA POLITECNICA, AV ESCOLA POLITECNICA, AV DR ANTONIO MARIA LAET, AV DR ANTONIO MARIA LAET, RUA PARANABI, RUA ARARITAGUABA, RUA ARARITAGUABA, AV DO POETA	http://www.cetsp.com.br/noticias/2015/11/16/cet-implanta-reducao-de-velocidade-maxima-em-mais-10-vias.aspx
11/19/2015	11/25/2015	AV S GABRIEL, AV SANTO AMARO, AV JOAO DIAS, AV ADOLFO PINHEIRO, RUA RHONE, AV ADUTORA DO RIO CLARO	http://www.cetsp.com.br/noticias/2015/11/19/cet-implanta-reducao-de-velocidade-maxima-em-mais-6-vias.aspx
11/23/2015	11/27/2015	AV MIGUEL IGNACIO CURI, RUA CASTELO DO PIAUI, AV RAGUEB CHOHFI, ES IGUATEMI	http://www.cetsp.com.br/noticias/2015/11/23/cet-implanta-reducao-de-velocidade-maxima-em-mais-5-vias.aspx
11/27/2015	12/2/2015	AV PAES DE BARROS, RUA TAQUARI, RUA BRESSER, VD BRESSER, AV BERNARDINO BRITO FONSECA DE CA, AV BERNARDINO BRITO FONSECA DE CA, AV PROF EDGAR SANTOS, AV PROF EDGAR SANTOS, AV ITAQUERA	http://www.cetsp.com.br/noticias/2015/11/27/cet-implanta-reducao-de-velocidade-maxima-em-mais-8-vias.aspx
12/2/2015	12/4/2015	AV PIRES DO RIO, AV DEP JOSE ARISTODEMO PINOTTI, AV DEP JOSE ARISTODEMO PINOTTI, ES DO IMPERADOR, ES DE MOGI DAS CRUZES, RUA EMBIRA, AV S MIGUEL	http://www.cetsp.com.br/noticias/2015/12/02/cet-implanta-reducao-de-velocidade-maxima-em-mais-6-vias.aspx
12/4/2015	12/9/2015	RUA DR ASSIS RIBEIRO, AV VER ABEL FERREIRA, RUA BRIG GAVIAO PEIXOTO, RUA MONTE PASCAL, VD DOMINGOS DE MORAES, AV GAL EDGAR FACO	http://www.cetsp.com.br/noticias/2015/12/04/cet-implanta-reducao-de-velocidade-maxima-em-mais-6-vias.aspx
12/9/2015	12/11/2015	AV INAJAR DE SOUZA, AV INAJAR DE SOUZA, AV COMEN MARTINELLI, AV ERMANO MARCHETTI, AV MARQ DE SAO VICENTE, RUA SERGIO TOMAS, RUA NORMA PIERUCCINI GIANNOTTI, AV RUDGE, VD ENG ORLANDO MURGEL, AV RIO BRANCO, AV ORDEM E PROGRESSO, PTE JULIO DE MESQUITA NETO, AV NICOLAS BOER, VD POMPEIA, AV ALEXANDRE COLARES, AV MANOEL MONTEIRO DE ARAUJO, AV DOMINGOS DE SOUZA MARQUES, AV ALM DELAMARE, RUA ANCHIETA, RUA FUNCHAL, AV CHEDID JAFET	http://www.cetsp.com.br/noticias/2015/12/09/cet-implanta-reducao-de-velocidade-maxima-em-mais-24-vias.aspx
12/14/2015	12/16/2015	AV SARG MIGUEL DE SOUSA FILHO, AV TTE AMARO FELICISSIMO DA SILVEIRA, AV TTE AMARO FELICISSIMO DA SILVEIRA, AV SERAFIM GONCALVES PEREIRA, AV MORUMBI	http://www.cetsp.com.br/noticias/2015/12/14/cet-implanta-reducao-de-velocidade-maxima-em-mais-4-vias.aspx
12/16/2015	12/18/2015	RUA MANOEL BARBOSA, AV RAIMUNDO PEREIRA DE MAGAL., RUA PRINCIPAL(PERUS), RUA GUIDO CALOI, AV GIOVANNI GRONCHI, ES DO ALVARENGA, RUA DR JOSE MARIA WHITAKER, RUA ALVINOPOLIS, AV ANTONIO BATUIRA, AV QUEIROZ FILHO, RUA CERRO CORA, RUA CERRO CORA, RUA CERRO CORA, RUA CONS MOREIRA DE BARROS, RUA MAUA	http://www.cetsp.com.br/noticias/2015/12/16/cet-implanta-reducao-de-velocidade-maxima-em-mais-14-vias.aspx
12/16/2015	12/18/2015	AV DUQ DE CAXIAS	http://capital.sp.gov.br/noticia/cet-implanta-reducao-de-velocidade-maxima-em-mais-14-vias
12/30/2015	12/29/2015	AV LUIZ GUSHIKEN	http://www.cetsp.com.br/noticias/2015/12/30/cet-implanta-reducao-de-velocidade-na-avenida-luiz-gushiken.aspx

Figure A.2: Banners indicating an upcoming speed limit reduction in 2015

Table A.2: Parameters for Accident Costs Components

	Accident Severity		
	No Injuries	With Injuries	With Fatalities
Vehicle Costs			
Auto	7,159	12,127	19,324
Motorcycle	2,473	2,741	4,270
Bike	0	169	124
Utility Veh.	10,570	20,240	35,091
Truck	22,314	65,656	47,825
Bus	16,069	10,537	20,686
Other	10,307	80,109	81,209
Victims Cost			
Unharmed	1,086	4,111	1,840
Injured	14,439	66,802	74,896
Fatality	-	-	3,862,030

Notes: All values are presented BRL of 2015. All parameters were extracted from Carvalho et al. (2015), except Fatal Victims Costs, for which the paraeter comes from Viscusi & Masterman (2017)

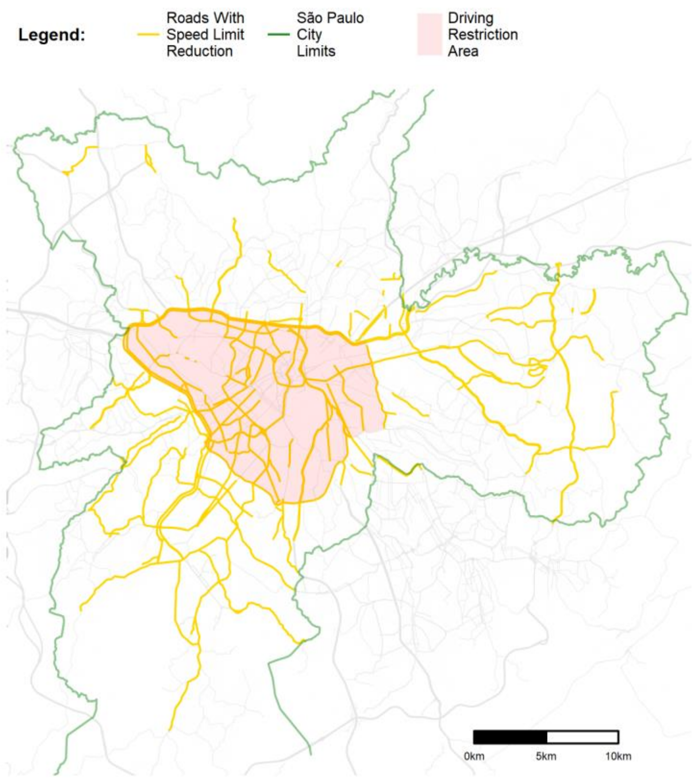

Figure A.3: Road Segments with Speed Limit Reductions in 2015 and the area of driving restriction in São Paulo.

Appendix B: Chapter 2 Appendix

Table B.1: AAPs Adopted by Brazilian Federal Universities (2004-2013)

University	Type of AAP	Value	Eligibilibity Criteria	Year of implementation	Description
UFABC	quotas	50%	phs blacks on phs indigenous on phs	2007	Starting in 2007 (when the university was created), UFABC reserved 50% of its positions to students who graduated from public secondary schools. Out of this total, 27.2% (13.6% of total) was reserved for black students and 0.4% (0.2% of total) to indigenous.
UFAC	none			2013	Until 2012 UFAC did not have any type of AAP in their selection process.
UFAL	quotas	20%	blacks black women	2005	UFAL reserved 20% of its positions to students who self-declare as black. Starting in 2006, 60% of this quota was restricted to black women.
UFAM	none			2013	Until 2012 UFAM did not have any type of AAP in their selection process.
UFBA	quotas	45%	phs blacks on phs indigenous on phs	2005	Starting in 2005, UFBA started a program of quotas where 36.55% of its positions were reserved to black students from public high schools, 6.45% were reserved to all public high school students and 2 % were reserved to indigenous.
UFC	none			2013	Until 2012 UFC did not have any type of AA police in their selection process
UFCG	none				Until 2012 UFCG did not have any type of AA police in their selection process
UFES	quotas	40 - 45%	phs	2008	Starting in 2008, UFES reserved 40% of its positions to students who did 4 years of primary education and all secondary education on public high schools. This share was increased to 45% in 2009 and was kept at that level until 2012.
UFF	bonus	10-20%	phs	2008	UFF granted a 10% bonus on the exam score of students who graduated from public secondary education. The bonus was increased to 20% in 2012.
UFG	quotas	20%	phs blacks on phs	2005	UFG reserved 10% of its positions to public high school students and additional 10% to black students from public high schools.
UFJF	quotas	30 - 50%	phs blacks on phs	2006	Starting in 2006, UFJF reserved 30% of its positions to students from public high school. Out of this share, 50% (25% of total) was reserved for black students. The total share was increase to 40% in 2007 and 50% in 2008, and remained at that level until 2012.
UFLA	none			2013	Until 2012 UFLA did not have any type of AA police in their selection process.
UFMA	quotas	50%	phs blacks blacks on phs	2007	Starting in 2007, UFMA reserves 25% of its positions to students from public high school and 25% to black students regardless of their type of high school. In 2009 the share reserved for black students was restricted to students who completed secondary education on public high schools. The policy remained the same until 2012.
UFMG	bonus	10 - 15%	phs blacks on phs	2009	UFMG granted a bonus of 10% to the score of students who completed all secondary education and the last 4 years of primary education on public high schools. Additionally, the students who satisfied the above criteria and were self-declared as black, would gain a total bonus of 15%.

Table B.1: AAPs Adopted by Brazilian Federal Universities (2004-2013) — (Continuation)

University	Type of AAP	Value	Eligibility Criteria	Year of implementation	Description
UFMS	none			2013	Until 2012 UFMS did not have any type of AAP in their selection process.
UFSM	quotas	36-40%	phs blacks indigenous handicapped	2008	Starting in 2008, UFSM reserved its positions according to the following system: 11% for black students, 5% for handicapped, 20% for public schools and up to 8 new positions for indigenous. The system remained roughly the same until 2012 with a yearly increment of 1% in the share reserved for black students.
UFMT	quotas	50%	phs blacks on phs	2012	Starting in 2012, UFMT created a system of quotas where 50% of its positions were reserved for public high school students. Out of this total, 40% (20% of total) will be reserved to black candidates.
UFOP	quotas	30%	phs	2009	Starting in the second semester of 2008, UFOP started reserving 30% of its positions to students who completed all secondary education on public schools.
UFPA	quotas	50%	phs blacks on phs	2006	The policy at UFPA reserved 50% of its positions to students who completed all secondary education on public high schools. 40% of this share (20% of total) was reserved for students who declared themselves as black (*negro*). In both cases, students had to opt in.
UFPB	quotas	25 - 30%	phs	2006	Starting in 2006, UFPB reserved 25% of its positions to students from public secondary schools. In 2012 this share was increased to 30%. Official notices for those exams could not be found.
UFPE	bonus	10%	phs	2005	Starting in 2005, UFPE granted a bonus of 10% on the score of students who completed the whole secondary education on public high schools on the surrounding areas of each of its campi. The policy remained mostly the same until 2012 with some changes about the geographical areas in each vestibular. Formal information such as notices and exam calls is really hard to find for UFPE.
UFPEL	none			2013	Until 2012, UFPEL did not have any type of AAP in their selection process.
UFPI	quotas	5 - 20%	phs blacks on phs	2007	In 2007, UFPI reserved 5% of its positions to students who completed all primary and secondary education on public schools. In 2008 this share was increased to 20% and remained like that until 2012.
UFPR	quotas	40%	phs blacks	2005	Starting in 2005, UFPR started a program of quotas where 20% of its positions were reserved to students from public high schools (with up to one year studied on private schools) and 20% were reserved to black students. The policy remained mostly the same until 2012, except for some changes in the required number of years studied in public schools. For the process of 2012 students were required to have studied all secondary and primary education to be eligible to the 20% of positions reserved for public schools.
UFRB	quotas	45%	phs blacks on phs indigenous on phs	2006	Founded in 2006, UFRB had its entrance exams carried by UFBA until 2010, hence it followed the same system of quotas of UFBA. After that, they continued following the same system of quotas of UFBA.

Table B.1: AAPs Adopted by Brazilian Federal Universities (2004-2013) — (Continuation)

University	Type of AAP	Value	Eligibility Criteria	Year of implementation	Description
UFRGS	quotas	30%	phs blacks on phs	2008	UFRGS reserved 30% of its positions to students who completed the whole secondary and primary education on public high schools. Addionally, 50% of this positions (15% of total), were reserved to students who self-declared as black.
UFRN	bonus	by program	phs	2006	Since 2006 grants a score bonus on the admission exam for students from public high schools from that state. The bonus is individually defined for each academic program.
UFRPE	bonus	10%	phs	2005	Starting in 2005, UFRPE granted a bonus of 10% on the score of students who completed the whole secondary education on public high schools of the countryside of the state. The policy remained mostly the same until 2012 with some changes about the geographical areas in each vestibular. Formal information from official notices were not found.
UFRR	none			2013	Until 2012 UFRR did not have any type of AA police in their selection process.
UFRRJ	bonus	10%	phs	2010	Starting in 2010, UFRRJ started giving a bonus of 10% to students who completed the whole secondary education on public schools.
UFS	quotas	50%	phs blacks on phs	2010	Starting in 2010, UFS started selecting 50% of its student from individuals who completed the whole secondary education and at least 4 years of primary education on public schools. Out of this share, 70% (35% of total) is reserved to students who self declare as black.
UFSC	quotas	30%	phs blacks on phs	2008	UFSC program of quotas had 20% of its positions were reserved to public high school students and 10% additional positions were reserved to black students from public high schools
UFSCAR	quotas	20 - 40%	phs blacks on phs	2008	Starting in 2008, UFSCAR started a program of quotas where 20% of its positions were reserved for students who studied the whole secondary education on public schools. Out of this share, 35% (7% of total) was reserved to black students from public high schools. In 2011 the total share was increased from 20% to 40%. The policy remained the same until 2012. Official exam calls for the period were not found.
UFSJ	quotas	50%	phs blacks on phs indigenous on phs	2010	Starting in 2010, UFSJ started reserving 50% of its positions to tstudents who completed primary and secondary education on public schools. Within this quota, a share corresponding to the proportion of each race in the state of Minas Gerais was reserved for each race. Therefore, an additional quota of 46% (23% of total) was reserved for black, brown and indigenous students from public schools.
UFT	quotas	5%	indigenous	2005	UFT reserved 5% of its positions to indigenous individuals. Until 2013, no other AA policy was introduced
UFTM	bonus	10%	phs	2009	Starting in the second half of 2009, UFTM started granting a bonus of 10% to students who completed 4 years of primary education and all secondary education on public schools.

Table B.1: AAPs Adopted by Brazilian Federal Universities (2004-2013) — (Continuation)

University	Type of AAP	Value	Eligibilibity Criteria	Year of implementation	Description
UFU	quotas	25 - 50%	phs	2011	Starting in 2008, UFU created a specific vestibular for students from public high schools who studied at least 4 years of primary education on public schools. This vestibular would consist of 3 exams taken once a year during secondary education. Therefore, the first students selected through this process would enter university in 2011. The share of positions selected through this process ranged from 25%-50%. The system faced a series of judicial disputes and different court orders had varying verdicts about the validity of reservations.
UFV	none			2013	Until 2012 UFV did not have any type of AAP in their selection process.
UnB	quotas	20%	blacks	2004	UnB was the first federal university to start a program of quotas in Brazil. Starting in the second semester of 2004, UnB reserved 20% of regular vestibular positions to black students.
Unifal	none			2013	Until 2012 Unifal did not have any type of AA police in their selection process.
UNIFAP	none			2013	Until 2012 UNIFAP did not have any type of AAP in their selection process.
Unifei	none			2013	Until 2012 Unifei did not have any type of AAP in their selection process.
Unifesp	quotas	10%	blacks on phs indigenous on phs	2005	Unifesp system of quotas reserved 10% to black or indigenous students who completed all secondary education on public schools
Unirio	none			2013	Until 2012 Unirio did not have any type of AAP in their selection process.
Univasf	quotas	50%	phs	2010	In 2010, Univasf started reserving 50% of its positions to students from public high schools.
URG (FURG)	bonus	6 - 10%	phs blacks on phs	2010	Starting with the exam of 2010, FURG granted a bonus of 6% to the score of students who completed at least 2 years of their secondary education on public high schools. Additionally, the students who satisfied the above criteria and were self-declared as black, would gain an additional of 3%. The system was slightly changed in 2011 with the following system of bonus: 4% for students from public school and additional 6% for black students from public high schools. The system was the same in 2012.
UTFPR	quotas	50%	phs	2008	UFTPR reserved 50% of its positions to students who completed the whole secondary education on public high schools.

Table B.2: Sample of Freshmen by University and ENADE Academic Group (2004-2010)

	Group 1			Group 2		Group 3		
University	2004	2007	2010	2005	2008	2006	2009	Total
UFRJ	387	467	1,115	1,214	2,205	674	1,468	7,530
UFPE	379	508	854	1,069	1,453	860	2,090	7,213
UFF	347	478	1,327	1,197	1,792	770	1,213	7,124
UFPA	254	435	852	1,557	1,904	742	1,116	6,860
UFRGS	466	388	946	862	1,471	594	1,581	6,308
UFBA	265	520	1,181	1,183	1,390	677	990	6,206
UFSC	325	323	769	1,124	1,371	600	1,628	6,140
UFMG	464	436	1,284	903	1,301	510	1,240	6,138
UFMT	532	534	1,001	1,237	1,337	398	980	6,019
UFPI	277	388	1,189	443	1,746	310	1,654	6,007
UFG	443	555	1,077	882	1,477	522	893	5,849
UFRN	343	338	906	975	1,354	562	1,152	5,630
UFAM	268	468	974	693	1,485	383	1,220	5,491
UFPB	427	413	952	664	1,177	615	1,216	5,464
UnB	325	354	694	969	1,017	602	1,302	5,263
UFPR	377	484	1,080	649	997	491	1,035	5,113
UFAL	397	606	939	500	1,337	252	886	4,917
UFS	360	305	732	691	1,385	351	740	4,564
UFES	268	340	674	460	982	465	1,308	4,497
UFSM	378	610	864	651	875	336	748	4,462
UFMS	285	251	435	788	934	536	1,019	4,248
UFC	258	282	615	824	1,081	311	810	4,181
UFU	246	236	575	462	1,346	259	770	3,894
UFPEL	267	400	928	445	979	168	349	3,536
UFMA	115	276	437	521	895	433	634	3,311
UFCG	73	107	529	734	1,159	191	482	3,275
UFV	300	281	496	640	796	210	423	3,146
UFJF	264	202	497	472	764	277	521	2,997
UFRPE	182	469	826	375	794	98	131	2,875
UFT	145	146	299	557	944	239	420	2,750
UFRRJ	217	215	305	268	413	225	967	2,610
Unirio	136	168	436	208	286	465	641	2,340
UFSCar	149	152	211	507	1,022	94	139	2,274
URG	62	69	95	558	593	179	498	2,054
UFAC	178	153	304	361	785	97	138	2,016
Ufop	97	66	140	536	804	149	218	2,010
Unir	31	70	115	320	571	294	341	1,742
UTFPR	31	113	164	159	652	71	88	1,278
UFRR	60	62	117	198	312	208	313	1,270
UFSJ	31	25	0	307	497	143	252	1,255
Unifesp	117	328	690	0	0	31	49	1,215
Ufla	140	128	408	197	245	28	48	1,194
Univasf	0	228	273	88	235	79	178	1,081
UNIFAP	31	34	53	228	411	90	87	934
Unifal	113	129	236	38	119	0	0	635
UFTM	51	144	349	0	0	10	0	554
Unifei	0	0	0	159	301	30	40	530

Table B.3: Competitiveness Model

	Dependent variable:				
	Black[a]	PHSS[b]	Low-educ parents[c]	women	ENADE score[d]
$LT \times t_2 \times C_{low}$ [e,f,g]	0.018	0.018	0.088	-0.040	0.002
	(0.081)	(0.095)	(0.123)	(0.049)	(0.081)
$LT \times t_2 \times C_{med}$	0.100	0.038	0.008	0.054	0.030
	(0.062)	(0.071)	(0.096)	(0.038)	(0.063)
$LT \times t_2 \times C_{hig}$	0.186^{**}	0.407^{***}	0.312^{***}	0.021	0.142^{*}
	(0.058)	(0.068)	(0.094)	(0.036)	(0.060)
$ET \times t_2 \times C_{low}$	0.050	0.143	0.106	-0.011	0.126
	(0.071)	(0.083)	(0.109)	(0.044)	(0.074)
$ET \times t_2 \times C_{med}$	-0.022	0.162^{*}	0.080	0.008	0.153^{*}
	(0.068)	(0.079)	(0.105)	(0.042)	(0.070)
$ET \times t_2 \times C_{hig}$	0.047	0.469^{***}	0.298^{**}	-0.035	0.221^{**}
	(0.065)	(0.076)	(0.104)	(0.040)	(0.067)
$t_2 \times C_{low}$	0.062	0.126^{**}	-0.014	0.079^{***}	-0.091^{*}
	(0.038)	(0.043)	(0.057)	(0.023)	(0.038)
$t_2 \times C_{med}$	0.072	0.159^{**}	0.087	0.070^{**}	-0.088^{*}
	(0.043)	(0.050)	(0.067)	(0.026)	(0.044)
$t_2 \times C_{hig}$	0.004	-0.191^{***}	-0.267^{***}	0.042	-0.057
	(0.045)	(0.053)	(0.074)	(0.028)	(0.047)
Program FE	yes	yes	yes	yes	yes
Obs	1,300	1,326	1,230	1,372	1,380
Adj R^2	0.873	0.758	0.774	0.904	0.463

notes: $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$.

Coefficients can be interpreted as relative changes. E.g., a coefficient of 0.5 indicates an increase of 50%

[a] Black: refers to the combined group of Pretos and Pardos

[b] PHSS: Public High School Student (all years)

[c] Low-educ parents: none of the student's parents have studied beyond primary education

[d] ENADE score: standard deviations to the mean score of freshmen from federal university in a same major

[e] LT AAP: Programs which had no AAP in 2005-2007, but adopted it in 2008-2010

[f] Post: dummy indicating the second cycle of ENADE exams (2008, 2009 and 2010)

[g] Program Competitivenss was defined based on the minimum SISU score of 2016. For details, see Figure 1.

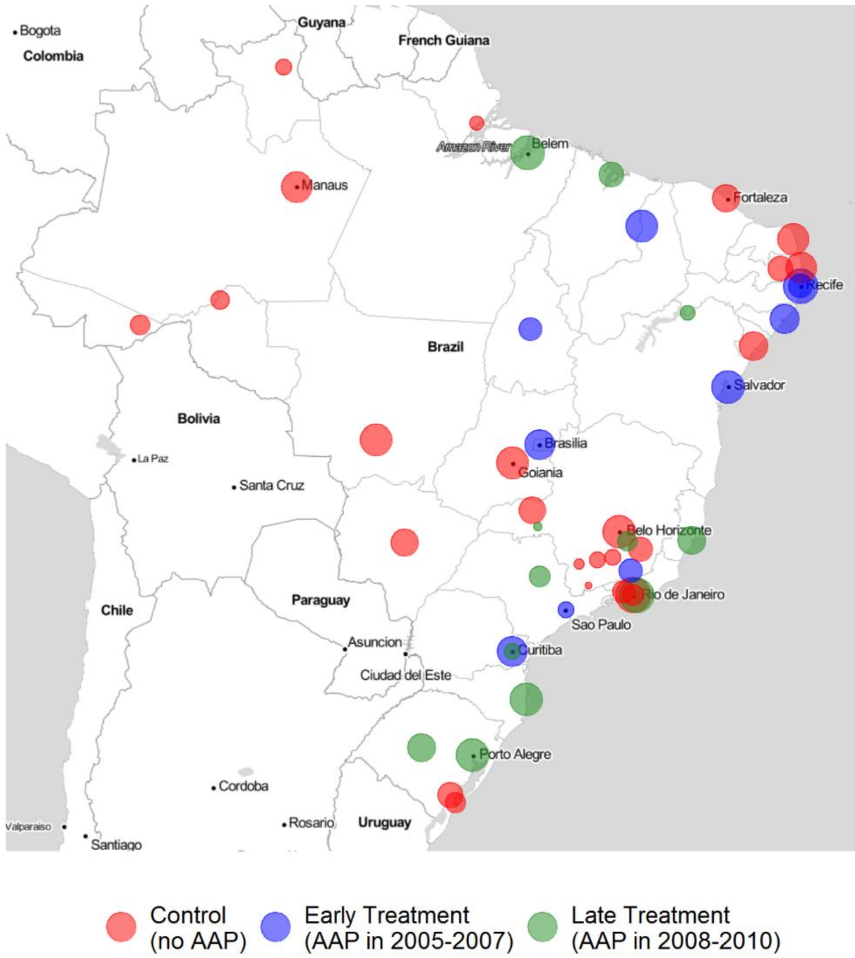

Figure B.1: Brazilian Federal Universities by year of AAP adoption (2005-2010)

Appendix C: Chapter 3 Appendix

Table C.1: Individual Characteristics by Survey

	São Paulo						Belo Horizonte		
	2007			2012			2012		
	obs.	× pop. weights	share[a]	obs.	× pop. weights	share[a]	obs.	× pop. weights	share[a]
Sex									
female	49,116	10,286,385	52.7%	12,898	10,443,713	52.2%	52,743	2,557,215	52.7%
male	42,289	9,248,235	47.3%	11,636	9,567,990	47.8%	47,913	2,290,945	47.3%
Age									
0 - 19	22,062	5,429,047	27.8%	6,381	5,507,141	27.5%	26,423	1,258,815	26.0%
20 - 39	29,249	6,413,396	32.8%	7,890	6,415,779	32.1%	31,699	1,535,324	31.7%
40 - 59	24,331	5,099,146	26.1%	6,366	5,152,632	25.7%	26,078	1,254,179	25.9%
60 - 79	13,048	2,259,223	11.6%	3,249	2,497,271	12.5%	13,877	674,915	13.9%
80 - +	2,715	333,809	1.7%	648	438,880	2.2%	2,579	124,928	2.6%
Work Status									
working	44,412	8,888,409	45.5%	12,199	9,711,942	48.5%	41,963	2,032,936	41.9%
retired	13,125	2,412,667	12.4%	3,167	2,449,236	12.2%	14,975	725,842	15.0%
unemployed	6,500	1,661,602	8.5%	1,612	1,391,430	7.0%	8,136	387,030	8.0%
never worked	6,717	1,707,149	8.7%	1,659	1,440,879	7.2%	1,034	49,910	1.0%
domestic worker	7,082	1,726,877	8.8%	1,773	1,511,509	7.6%	8,409	399,261	8.2%
student	13,569	3,137,917	16.1%	4,124	3,506,707	17.5%	16,043	773,178	15.9%
Education									
none	17,429	4,627,783	23.7%	4,552	4,003,569	20.0%	12,566	590,675	12.2%
incomplete primary	17,222	4,258,664	21.8%	4,867	4,164,771	20.8%	33,284	1,568,430	32.4%
primary	13,683	3,253,280	16.7%	4,163	3,558,635	17.8%	18,280	876,177	18.1%
secondary	25,370	5,514,262	28.2%	7,237	5,912,308	29.5%	27,221	1,341,608	27.7%
tertiary	17,701	1,880,630	9.6%	3,715	2,372,420	11.9%	9,305	471,270	9.7%

Notes: [a] shares are based on the number of observations multiplied by population weights

Table C.2: Mode Share and Trip Motivation by Survey

	São Paulo						Belo Horizonte		
	2007			2012			2012		
	obs.	× pop. weights	share[a]	obs.	× pop. weights	share[a]	obs.	× pop. weights	share[a]
Total Trips	169,665	38,094,385	100%	46,861	43,715,466	100%			
Mode Share									
public transportation									
bus - capital	23,808	5,644,866	14.8%	6,672	5,494,837	12.6%	25,391	2,920,189	22.4%
bus - other cities	4,937	2,140,540	5.6%	1,812	2,622,634	6.0%			
bus - metropolitan	2,730	1,248,668	3.3%	550	1,265,811	2.9%			
subway	12,753	2,223,397	5.8%	4,392	3,218,989	7.4%	1,846	206,259	1.6%
train	2,471	815,177	2.1%	933	1,141,140	2.6%			
school bus	5,098	1,326,602	3.5%	1,950	2,011,132	4.6%	9,113	679,912	5.2%
charter bus	1,484	513,591	1.3%	313	389,427	0.9%			
private									
driving	44,584	7,276,263	19.1%	10,121	8,644,290	19.8%	26,063	2,278,429	17.4%
passenger	16,804	3,105,088	8.2%	4,077	3,706,649	8.5%	12,374	1,068,235	8.2%
taxi	1,149	90,686	0.2%	220	135,156	0.3%	894	87,530	0.7%
moto	2,582	721,156	1.9%	998	1,038,960	2.4%	6,476	547,185	4.2%
other									
walk	49,715	12,623,047	33.1%	14,504	13,708,189	31.4%	52,353	4,809,512	36.8%
bike	1,354	303,828	0.8%	252	267,788	0.6%	1,783	129,764	1.0%
other	196	61,475	0.2%	67	70,463	0.2%	4,247	332,703	2.5%
Trip Motivation [b]									
work	41,209	9,260,825	44.6%	12,066	11,168,873	46.2%	31,398	2,871,669	40.7%
education	27,589	6,894,413	33.2%	7,762	7,364,866	30.5%	20,676	1,859,108	26.3%
shopping	4,652	799,410	3.8%	1,106	1,057,847	4.4%	3,679	348,539	4.9%
leisure	5,419	882,036	4.2%	1,107	973,142	4.0%	4,759	460,154	6.5%
other	15,579	2,946,686	14.2%	4,022	3,588,715	14.9%	5,586	606,156	8.6%

Notes: [a] shares are based on the number of observations multiplied by population weights

[b] excluding return trips to home

Table C.3: Specific Characteristics of Transit and Walking Trips (2007 São Paulo Survey)

	2007		
	obs.	expanded	share
Walking Trips	49,715	12,623,047	100%
Motive for walking			
short distance	43,815	11,181,967	88.6%
expensive fare	2,218	635,216	5.0%
transit stop is far	515	139,876	1.1%
transit takes time at origin	447	103,078	0.8%
transit trip is too long	78	11,981	0.1%
transit is crowded	148	31,787	0.3%
physical activity	1,336	255,854	2.0%
other	1,158	263,289	2.1%
Transit Trips	53,281	13,912,842	100%
Who paid for the trip			
self-paid	22,833	5,341,783	38.4%
employer	18,394	5,532,548	39.8%
exempt	4,516	1,019,105	7.3%
other	501	151,012	1.1%
NA	7,037	1,868,395	13.4%

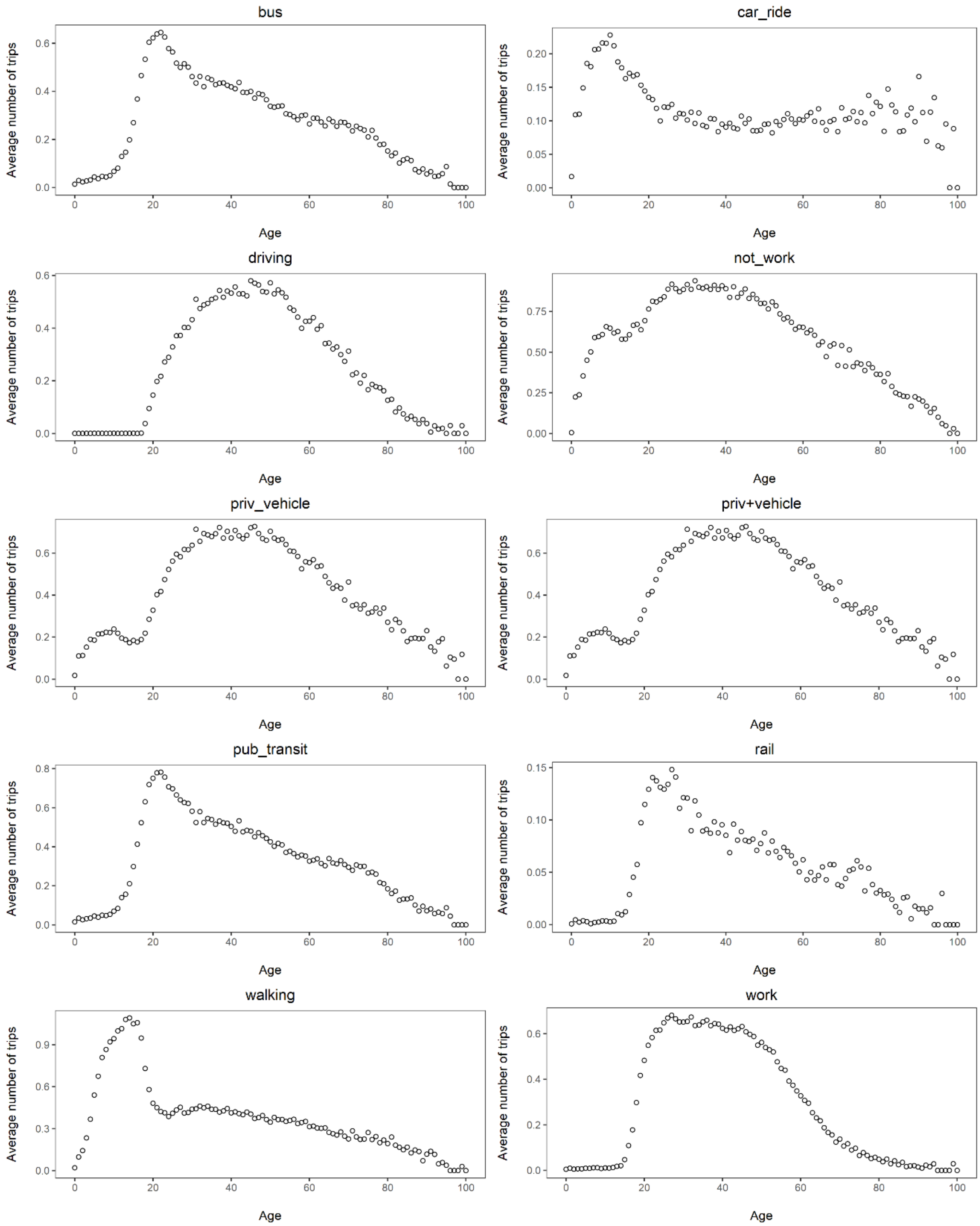

Figure C.1: Distribution of Covariates by Age

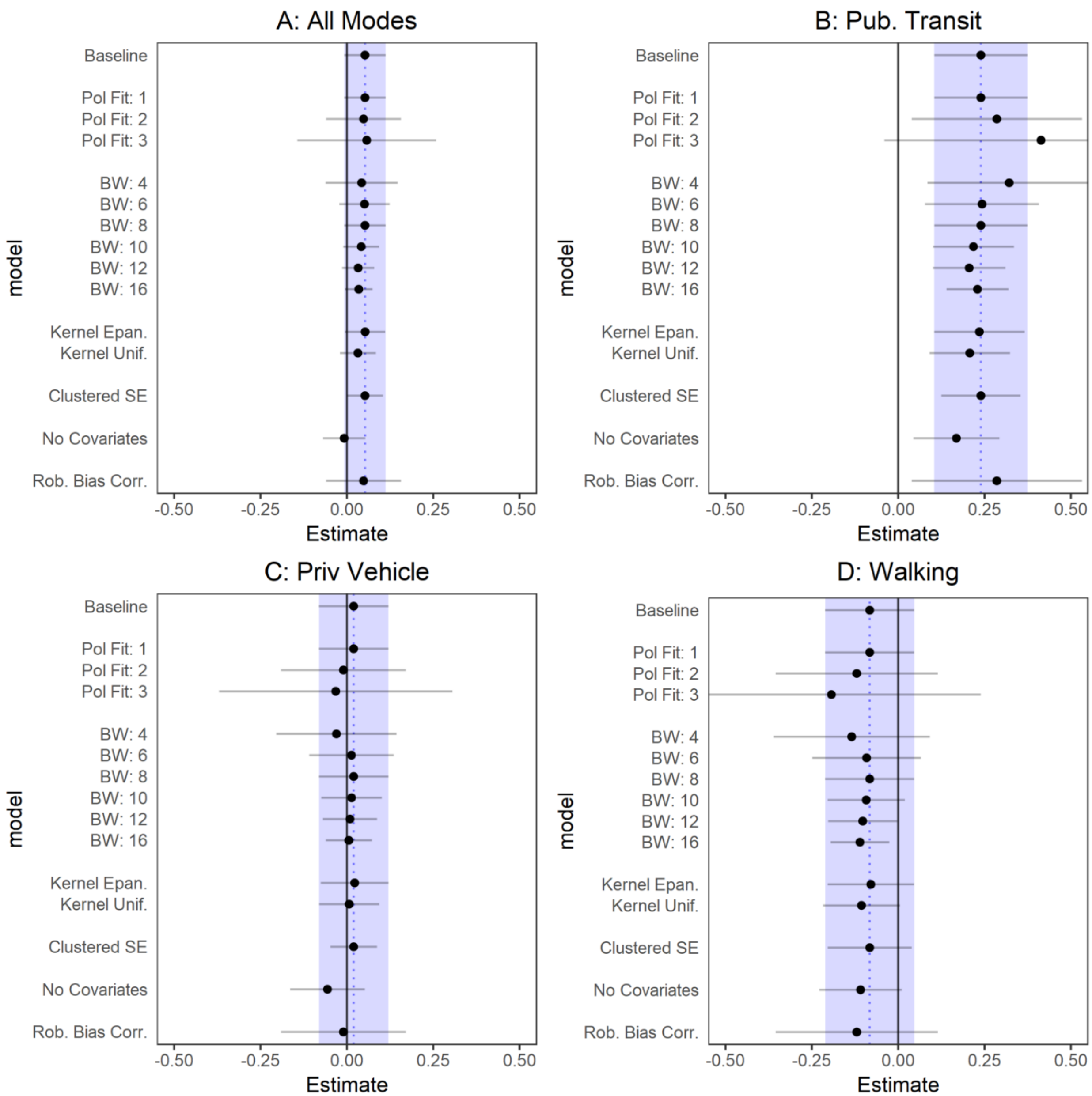

Figure C.2: Robustness of Results to Alternative RD Parameters

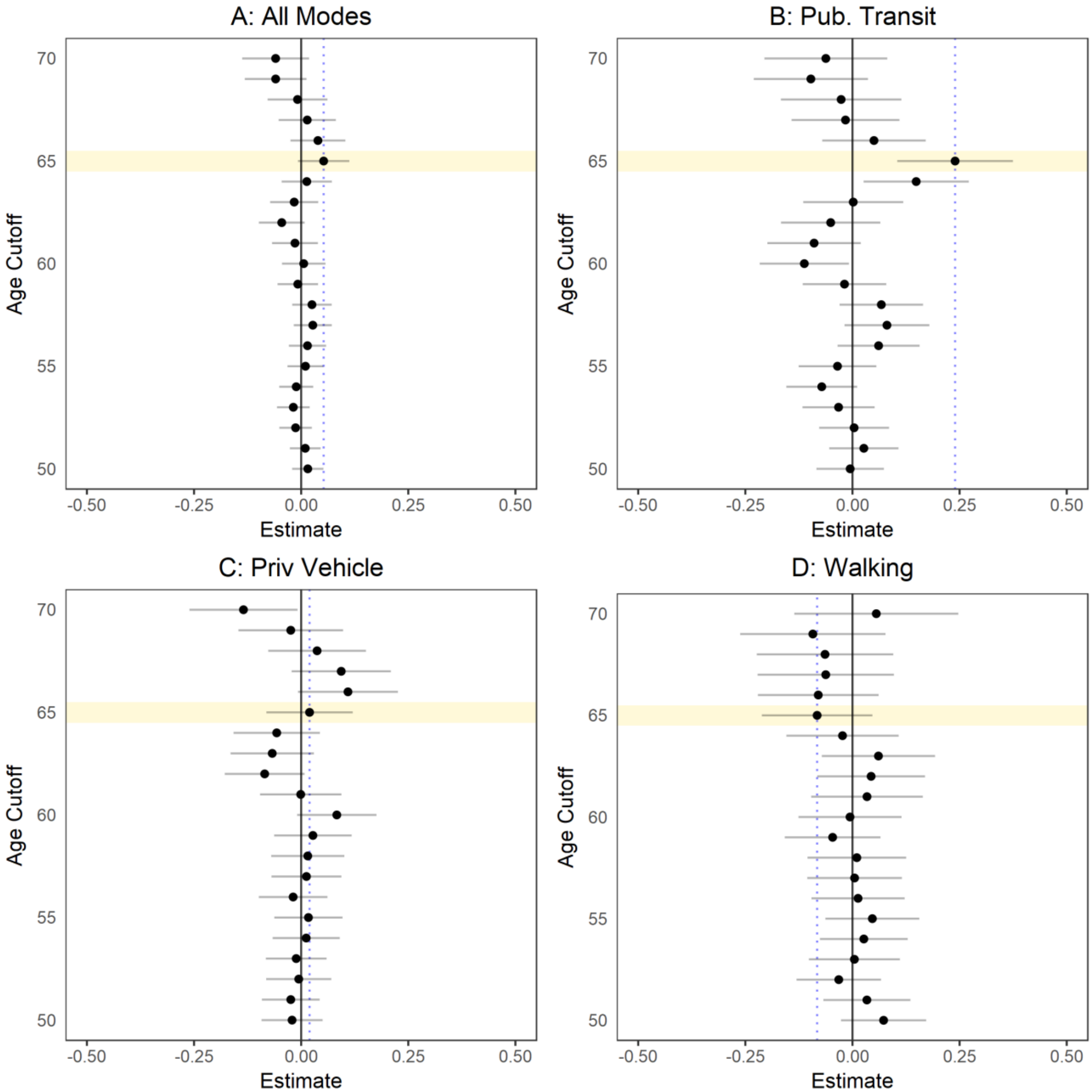

Figure C.3: Robustness of Results to Placebo Cutoff Ages

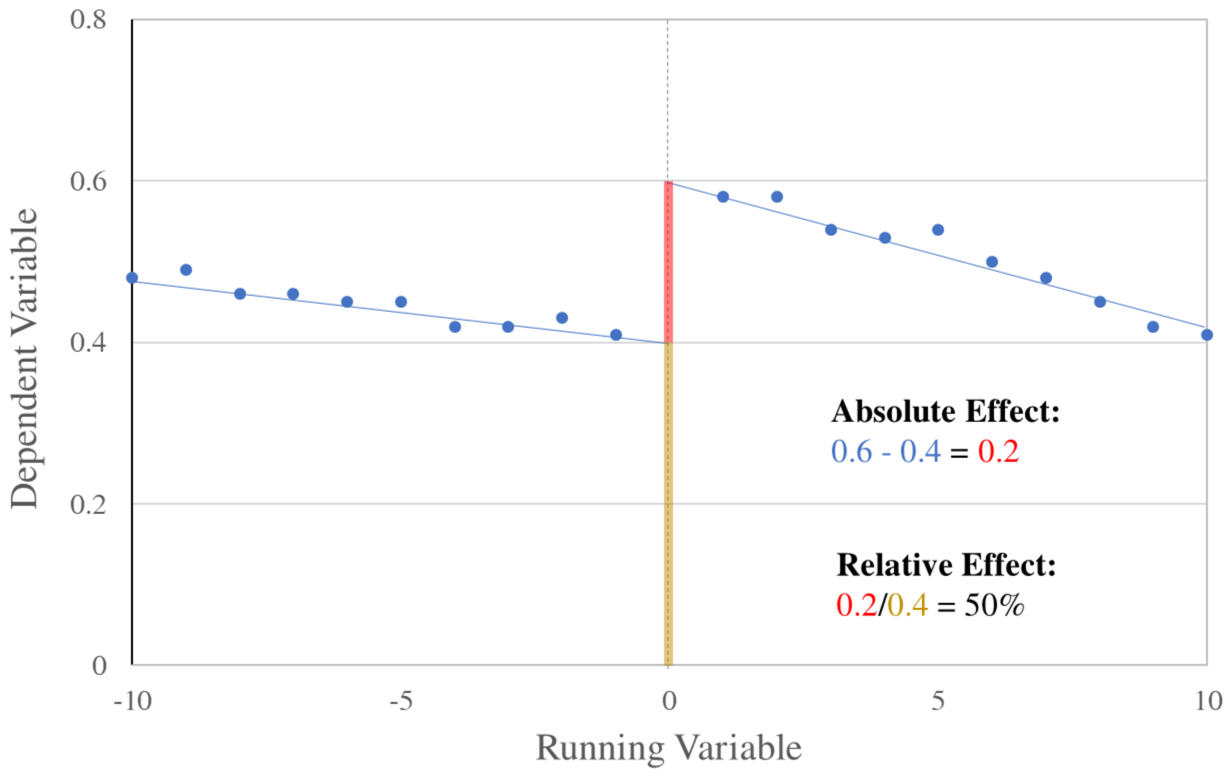

Figure C.4 Absolute and Relative RD Estimates

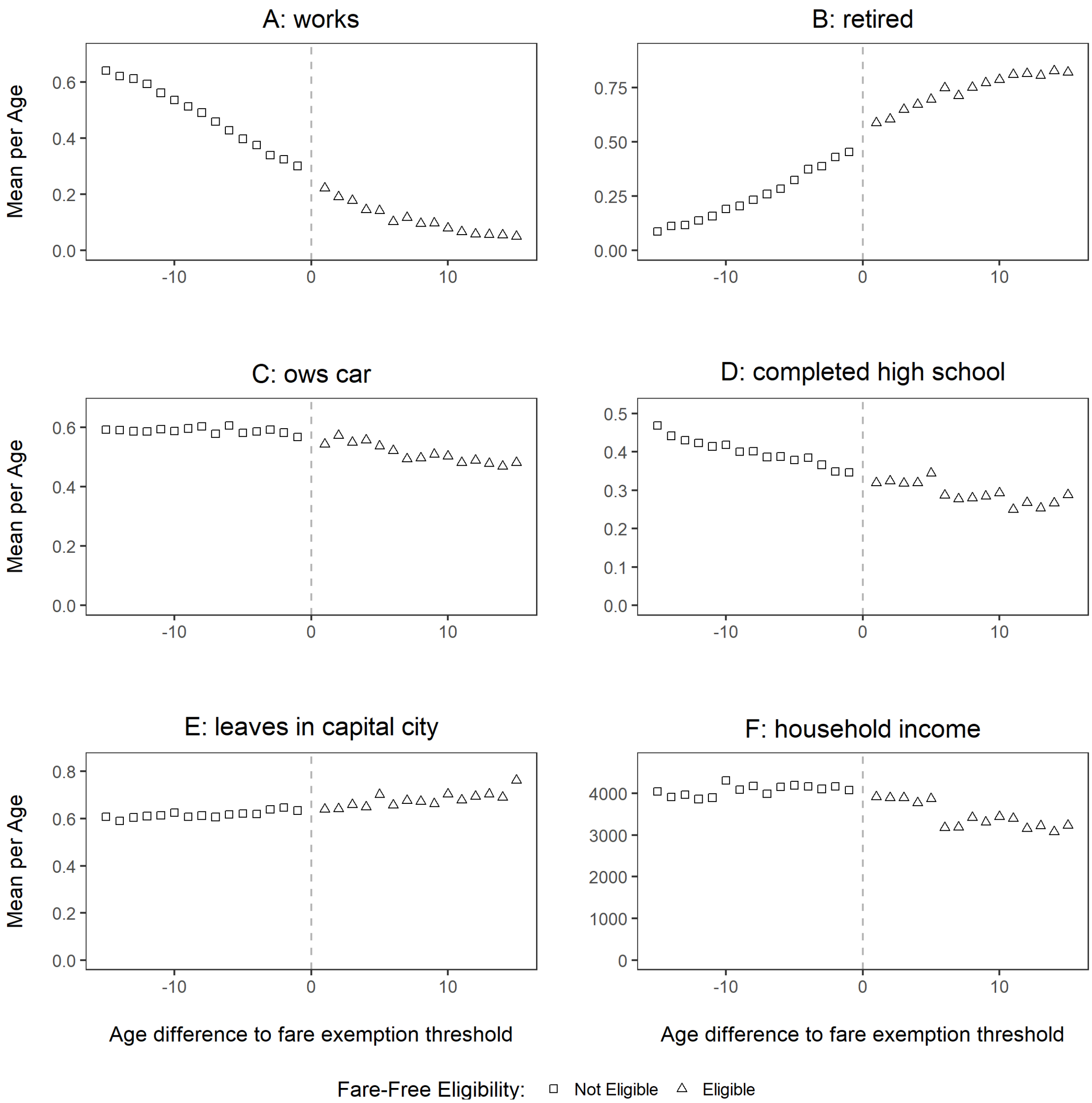

Figure C.5: RD Results for Covariates

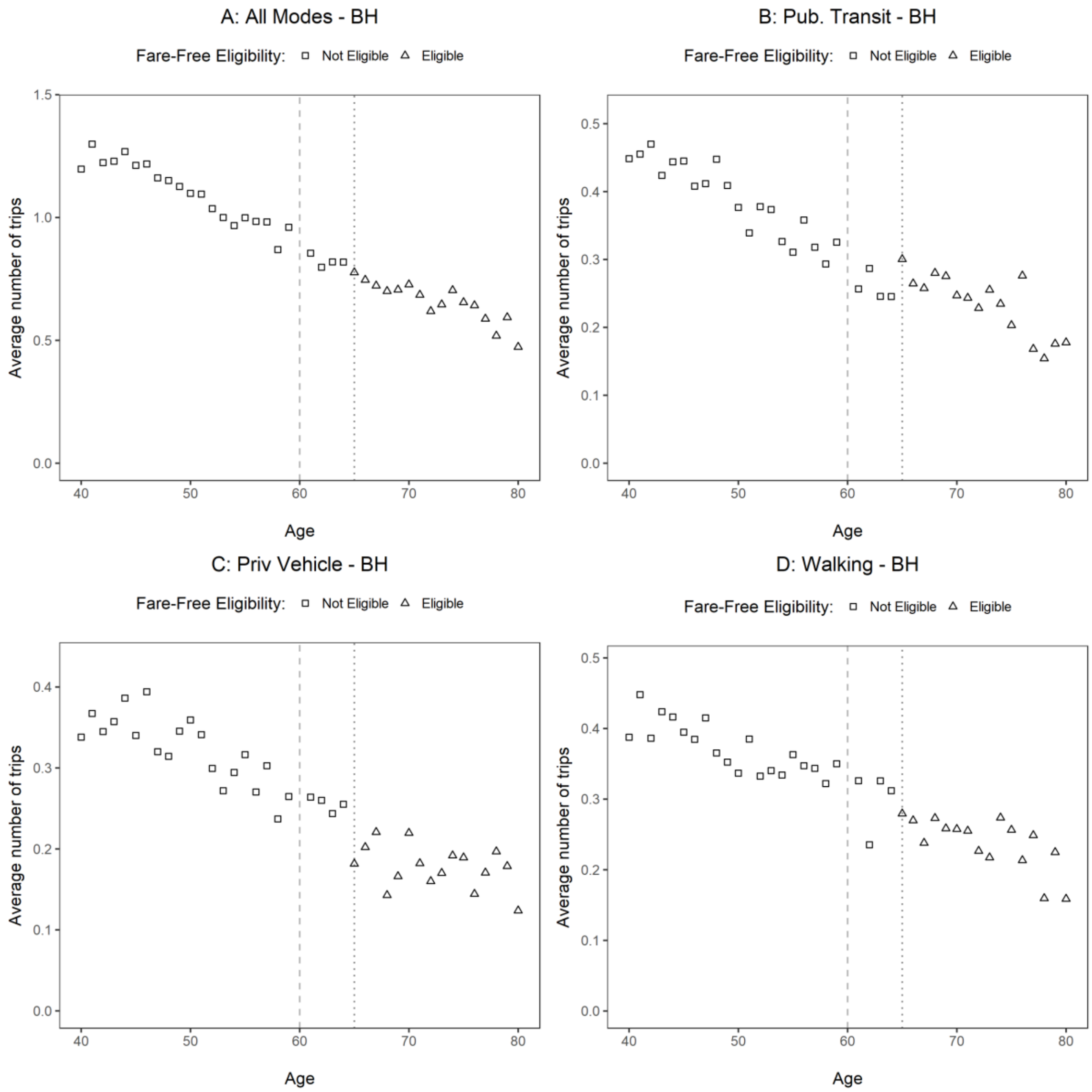

Figure C.6: RD Results for Travel Behavior Changes, Women from Belo Horizonte

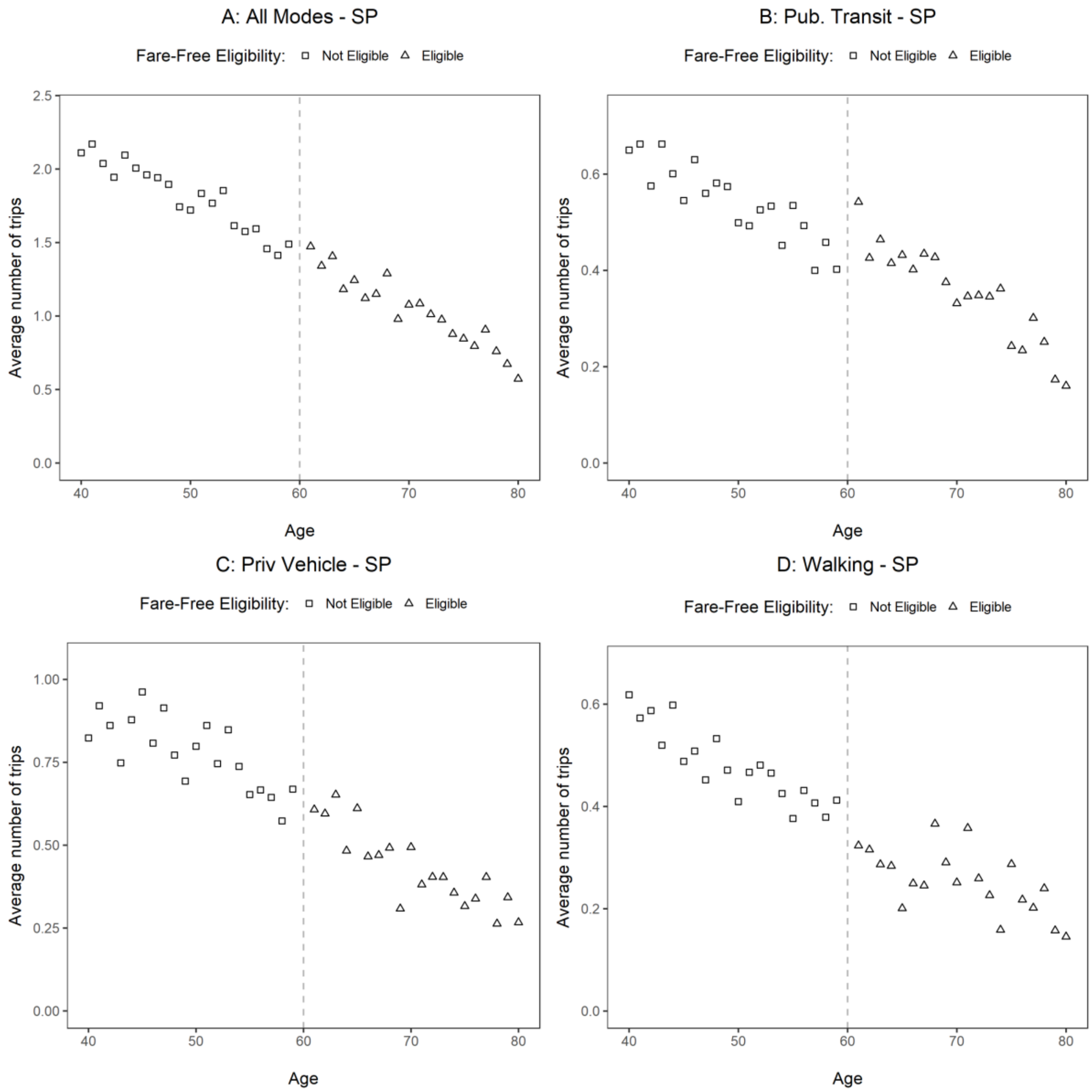

Figure C.7: RD Results for Travel Behavior Changes, Women from São Paulo